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The American Historical Review

THE AMERICAN HISTORICAL ASSOCIATION

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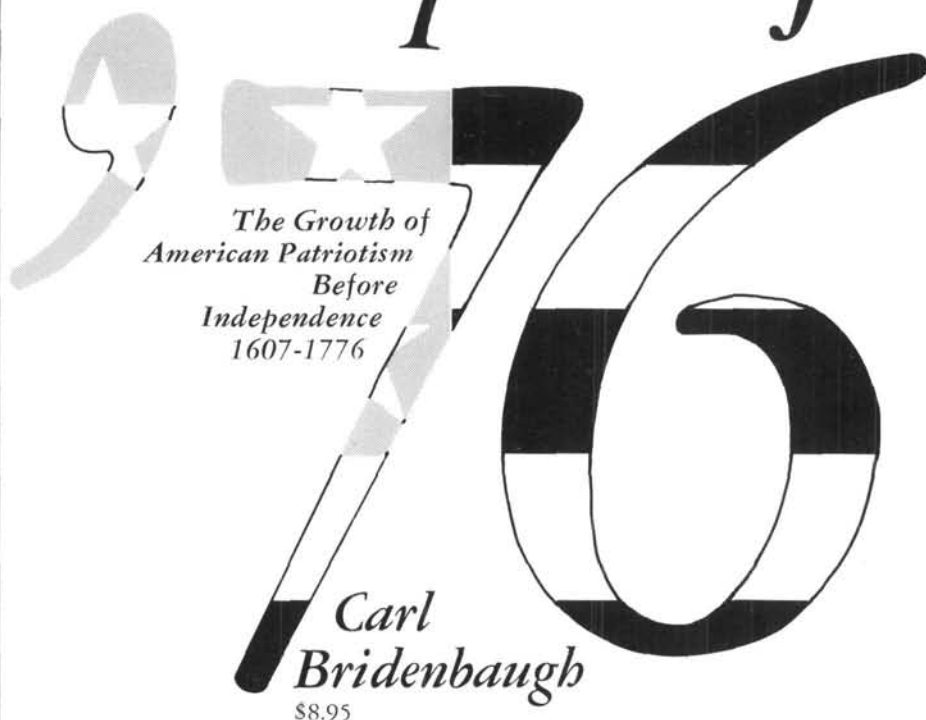


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The origins of the American Revolution.

The country's sense of national identity began with the first settlement at Jamestown and, argues Carl Bridenbaugh, was well established by the time the colonies declared their independence. To support this thesis, he examines the religious, ethnic, economic, and political forces that shaped distinctly American attitudes among nominally British colonists. This is a book which, *Library Journal* suggests, "should be required reading in this Bicentennial Era."

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Cover Illustration. Neighborhood women drilling in preparation for defending the Japanese mainland against American invasion, spring 1945. Photograph courtesy Kageyama Kōyō. (See Thomas R. H. Havens, "Women and War in Japan, 1937-45," pp. 913-34.)

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At its meeting on December 27, 1974, the AHA Council authorized the Executive Director to remove the Recently Published Articles (RPA) from the *American Historical Review* and to implement plans for a separate publication. The Council's action was based solely on economic factors and was necessitated by the rapid and enormous increases in production costs. As of February 1976, the RPA will be published separately, though it will continue to appear, as formerly, three times a year. The RPA will be bound and have a paper cover.

There will be a subscription charge, which for members of the Association will be \$5.00 for the three issues. The cost for institutions will be \$7.00 and for nonmembers \$8.00. The subscription charge will defray only some of the editorial and production costs. It is one way in which the Association will be able to continue to provide this valuable bibliography, which has no counterpart in the world.

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†GILBERT OSOFSKY was professor of history at the University of Illinois at Chicago Circle. In *Harlem: The Making of a Ghetto, 1890-1930* (New York, 1966) and other works, he examined racism in social and urban contexts, his major interest since graduate work under Robert Cross at Columbia University. In August 1974, death interrupted his study of abolitionist ideology. An obituary appeared in the April 1975 issue of the *AHR*.

THOMAS R. H. HAVENS teaches history at Connecticut College. The author of books on modern Japanese philosophy and agrarian nationalism in Japan, he is also the editor of "Patriotism in Modern History," scheduled for publication in 1976. Currently he is preparing a social history of Japan during World War II.

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Problems of Law and Order during 1790, the "Peaceful" Year of the French Revolution

SAMUEL F. SCOTT

STANDARD ACCOUNTS OF THE FRENCH REVOLUTION usually treat the year 1790, or even the period between October 1789 and the spring of 1791, as a time of peace and order in the otherwise tumultuous history of this great upheaval. Crane Brinton, Albert Goodwin, R. R. Palmer, and M. J. Sydenham in their treatment of the Revolution, for example, make almost no allusions to open conflict during this year.¹ For François Furet and Denis Richet, this was *l'année heureuse* or the "year of peace." In speaking of 1790, Alphonse Aulard has written, "This extraordinary year has been upheld as a year of national concord, as the best year of the Revolution, the year of fraternity." Although Aulard went on to argue that during the same time "the whole state politic was taken possession of by the middle class at the expense of the people," he made no mention of any conflicts more violent than parliamentary debates. French historians such as Albert Mathiez, Georges Lefebvre, and Albert Soboul, together with some English colleagues like J. M. Thompson and Norman Hampson, are more cognizant of persistent friction in 1790, but they too devote their attention almost exclusively to peaceful developments. Even the prominent historian of popular disturbances, George Rudé, maintains that "the eighteen months that followed [the violence of October 1789] were a period of comparative social calm."²

Grants from the Social Science Research Council, Wayne State University, and the American Philosophical Society contributed to support the research upon which this article is based, and I gratefully acknowledge this assistance.

¹ Crane Brinton, *A Decade of Revolution, 1789-1799* (New York, 1934), 40-53; Albert Goodwin, *The French Revolution* (New York, 1962), 90-108; R. R. Palmer, *The Age of Democratic Revolution*, vol. 1: *The Challenge* (Princeton, 1959), 489-502; M. J. Sydenham, *The French Revolution* (New York, 1966), 63-86.

² See François Furet and Denis Richet, *La Révolution*, vol. 1: *Des états généraux au 9 thermidor* (Paris, 1965), 136-83, and the slightly abridged English translation by Stephen Hardman, *The French Revolution* (London, 1970), 97-131; Alphonse Aulard, *The French Revolution: A Political History, 1789-1804*, vol. 1: *The Revolution under the Monarchy*, tr. Bernard Miall (New York, 1965), 213, and see pages 161-259 for Aulard's discussion of the whole period; Albert Mathiez, *The French Revolution*, tr. Catherine A. Phillips (New York, 1964), 58-118; Georges Lefebvre, *The French Revolution: From Its Origins to 1793*, tr. Elizabeth Moss Evanson (New York, 1962), 136-205; Albert Soboul, *Histoire de la Révolution française*, vol. 1: *De la Bastille à la Gironde* (Paris, 1962), 183-260; J. M. Thompson, *The French Revolution* (New York, 1966),



Fig. 1. Lesueur, *Parisian Army*. The Parisian National Guard, composed in part of former French Guards and other professional soldiers, effectively maintained peace in the capital through 1790. By kind permission of Madame Bidault de l'Isle. Photograph courtesy Bulloz.

In their treatment of events in 1790 the general histories of the Revolution written by these authorities, as well as the works of numerous other historians, concentrate on the great legislative accomplishments of the Assembly: the creation of a constitution, the reorganization of France's administrative and judicial systems, the reform of the Church, and the solutions to financial problems. It is, of course, impossible to deny the significance of these achievements, and it would be myopic to minimize their importance. Yet too exclusive an emphasis on these legal changes tends to obscure other significant developments during the same period. Both this concentration on the new legislation and the general view of 1790 as a time of peace and order are due, in part, to the attention that Paris has commanded from both contemporaries and later historians. After

112-287; Norman Hampson, *A Social History of the French Revolution* (Toronto, 1966), 86-131; George Rudé, *The Crowd in History: A Study of Popular Disturbances in France and England, 1730-1848* (New York, 1964), 109. Also see Pierre Gaxotte, *La Révolution Française* (Paris, 1928), 150-96; Leo Gershoy, *The French Revolution and Napoleon* (New York, 1964), 131-91; Louis Gottschalk, *The Era of the French Revolution* (Cambridge, Mass., 1957), 160-84; and Gaetano Salvemini, *The French Revolution, 1788-1792*, tr. I. M. Rawson (New York, 1962), 155-201.



Fig. 2. Lesueur, *Parisian Army, continued*. By kind permission of Madame Bidault de l'Isle. Photograph courtesy Bulloz.

the October Days, Paris not only resumed her role as the center of all political power but also, with the creation of the National Guard, became the best policed city in the country, indeed in all Europe. These developments both confirmed the unique character of the capital and enhanced its already prominent role in the nation. At the same time, Paris became all the more untypical of the conditions and problems that existed elsewhere in France. For, despite the peacefulness that reigned in the capital during 1790, other areas, both rural and urban, suffered from bitter tensions that often erupted into violent conflict in that year.

Although this article can make no claim to be a complete and systematic survey of violence throughout France during this period, the research upon which it is based offers conclusive evidence that the traditional and generally accepted view of this stage of the Revolution as a time of peaceful and orderly transition dominated by sentiments of fraternity is at least exaggerated and even erroneous for the country as a whole. As will be seen, many of the bitter internal divisions that were to plague France in the course of the next decade had already broken into violent conflict by the second year of the Revolution. Some of this violence was of a "traditional" nature—food riots, smuggling, brigandage—and arose from conditions en-

demic in eighteenth-century French society.³ A much more striking and significant type of conflict, however, developed out of the conditions created by the Revolution itself, and it is on these phenomena that this study will concentrate.

One of the most fundamental issues raised by the Revolution concerned the source of legitimate authority. Like other revolutionary upheavals, the French Revolution of 1789 challenged the previous regime, demanded a redistribution of political power, and disorganized the existing mechanisms for exercising that power.⁴ In contrast to subsequent revolutions in nineteenth-century France, however, the old regime was not replaced in 1789 but continued—at least in part—for the next three years in the person and powers of the king and his royal agents. In 1789 the legitimacy of the Old Regime was destroyed—but not totally. The revolutionary events of that year established a new legitimacy—but not completely. As a result the fragmentation of authority that naturally results from revolution was formalized and eventually sanctioned in the Constitution of 1791. This situation compounded the confusion about legitimate authority, encouraged disputes over its exercise, and led directly to the violent confrontations that made the problem of law and order such a severe one during 1790.

Nowhere was the question of legitimate authority posed in a more dramatic fashion or with more crucial consequences than in matters involving the major police agencies of the government—the regular army and the National Guard. Although the larger towns and cities of eighteenth-century France possessed their watches and guards and although the *maréchaussée*—a small rural constabulary—policed the countryside, the only forces capable of dealing with large-scale or widespread disorders were the Royal Army and the National Guard, the first being one of the primary bases of monarchical power and the other the arm of the revolutionary bourgeoisie.⁵ In the aftermath of 1789 the authority to employ these forces

³ Even an unsystematic investigation reveals such widespread disturbances over food during 1790 and early 1791 that one can conclude that in many regions of France—possibly in certain localities of all regions—segments of the population were unable, or feared they might be unable, to buy food. Such violence, requiring the employment of regular troops, or the National Guard, or both to restore order, threatened or occurred at Aix-en-Provence in January 1790 (Lieutenant Carnot, “Le Régiment de Lyonnais pendant la Révolution, 1789–1793,” p. 9, in *Historiques des Régiments d’Infanterie*, Archives de la Guerre [hereafter AG]), at Bourbon-Lancy, Bourbon-l’Archambault, Chavagne, and Thiel in Bourbonnais in May (Archives Nationales [henceforth AN], H 1453), at Salon in Provence in August (Carnot, “Régiment de Lyonnais,” 18), at Angers in September (AN, H 1453), at Saint-Malo also in September (AN, F736791), and at Laon, Chauny, Saint-Quentin, Ham, Noyon, Amiens, Dallon, Jussy, Fragnier, Roye, and Guise in Picardy during late 1790 and early 1791 (correspondence of General Biron in AG, B1* 208, 209). Smuggling also frequently led to violence. For example, detachments of the National Guard and dragoons had to be employed to halt illegal trade in contraband tobacco at Sillé-le-Guillaume near Le Mans in late March 1790 (AN, H 1453). Finally, throughout 1790 troops in neighboring departments were kept on constant alert to deal with “brigands” from the papal enclave of Avignon (AN, F736591). Examples like this are multiplied many times over in the archives.

⁴ See the article by James Rule and Charles Tilly, “1830 and the Unnatural History of Revolution,” *Journal of Social Issues*, 28 (1972): 49–76, which provided a theoretical framework for this study.

⁵ Greater attention will be devoted to the regular army than to the National Guard in this

was a bitterly disputed point among political rivals. Local officials contended with national authorities, and opposing local groups vied for the right to command law enforcement agencies. Intimately related to this controversy was the question of what forces to employ, regulars or guardsmen. Beyond this problem, divisions within each of these forces, between officers and men in the Royal Army and between opposing factions in the National Guard, seriously hampered the maintenance of order and sometimes completely paralyzed both civil and military officials. All too often the means of resolving these disputes over legitimate authority was violent conflict.

These conflicts fall, logically, into two broad categories: those which developed out of long-standing antagonisms but which took on a new character and intensity as a result of the political changes brought by the Revolution and those which emerged directly out of these changes and the struggle for power that they entailed. Both types had a common, fundamental source: rival claims to political legitimacy and the rights and powers that are its attributes. In the first category are included the numerous military problems that plagued the government in 1790. The soldiers of the Royal Army had long resented the harsh and inequitable regime imposed upon them by their aristocratic officers; but only in the atmosphere of general revolutionary change were they able to break with the firm discipline so deeply inculcated in them. Likewise, the hostility between Catholics and Protestants, which had caused so much suffering in France since the Reformation, was given new impetus and direction by the Revolution. And, although peasant hostility toward feudalism was one of the long-range causes of the Revolution, it found full expression and partial justification only in 1789 and after. Unlike these conflicts, which translated pre-existing hostilities into new, political terms, another form of conflict arose over problems—such as elections, control of the military, and the extent of local autonomy—that were engendered by the Revolution itself. Regardless of the specific issues in dispute, the resolution of all of these controversies depended on the crucial questions of who represented legitimate authority and what powers this included.

FOR THE REGULAR ARMY, 1790 was a year of disintegration. More than one-third of the units in the Royal Army suffered from insubordination between January and December of that year, an unprecedented situation.⁶ This insubordination ranged from excessive desertion, an essentially passive ex-

article because the ideas and conclusions upon which it is based developed originally from a rather different study, the response of the Royal Army to the Revolution. More extensive and intensive research on the National Guard, with its great local variations, is required before its role in the Revolution can be fully appreciated.

⁶ The general comments concerning insubordination in the Royal Army are based upon a study of seventy-three line units, approximately forty per cent of the total complement of the army, during the period 1788–93. These and other results of this study are presently being incorporated into a manuscript on the response of the Royal Army to the French Revolution.

pression common in the past, to large-scale mutiny; but in more than three-fourths of the instances the insubordination entailed some form of open rejection of the officers' authority by the soldiers under their command. This movement climaxed in the great mutiny at Nancy in late August when three line regiments engaged in a pitched battle with other regular and National Guard units sent to repress them. Although many incidents of insubordination did not immediately affect civil society, the general collapse of military discipline in 1790 was so serious that the government's ability to provide for national defense and to maintain domestic order was dangerously impaired. In addition, many other incidents of insubordination directly involved civilians.

In nearly forty-five per cent of the instances of military insubordination during 1790 the soldiers involved had friendly relations with civilian elements, a situation that directly or indirectly encouraged their defiance of their superiors. In January 1790, for example, the regiment of Vivarais Infantry was ordered to leave Béthune where it had been stationed for three years. Some three hundred men of the regiment, however, refused to obey and stayed in Béthune where they were supported in their decision by a number of citizens, including municipal officials.⁷ In April two infantry regiments stationed at Lille—the Royal Vaisseaux and La Couronne Infantry—engaged in an armed combat with the Normandie Chasseurs and Colonel-Général Cavalry, a conflict the officers of all four regiments were powerless to stop. The Lillois National Guard gave its support to the infantrymen, many of whom had associated with members of the local "patriot" club. Only an order signed by Louis XVI himself finally put an end to the fighting.⁸ Between April and August the commander and most of the personnel of the National Guard of Hesdin encouraged the men of the Royal Champagne Cavalry in their defiance of their officers during a mutiny led by a renegade junior officer, Louis Nicolas Davout, who later became Marshal of the Empire.⁹ At Perpignan in late May and early June the commander of the regiment of Touraine Infantry, the vicomte de Mirabeau, attempted to end fraternization between the men of his regiment and National Guardsmen and to restore the passive obedience of the Old Regime. Instead, the colonel was forced to flee his regiment and shortly thereafter was arrested by officials at Castelnaudary who acted on the basis of a complaint from his own troops.¹⁰ Although the underlying hostility between officers and men that led to these and other incidents had long

⁷ See Louis-François Quarré-Reybourbon, *Souvenirs Béthunois: Un Episode de la Révolution à Béthune* (Lille, 1886), 5-9; Charles Poisson, *L'Armée et la Garde Nationale* (Paris, 1858-62), 1: 213-14; and Lucien de Chilly, *Le premier Ministre constitutionnel de la Guerre, La Tour du Pin: Les Origines de l'armée nouvelle sous la Constituante* (Paris, 1909), 114-16.

⁸ Poisson, *Armée et Garde Nationale*, 1: 215-17.

⁹ Chilly, *La Tour du Pin*, 91-110; Henri Choppin, *Les Insurrections militaires en 1790* (Paris, 1903), 151-204.

¹⁰ Chilly, *La Tour du Pin*, 132-38; Poisson, *Armée et Garde Nationale*, 1: 227-29; Jacques Godechot, *Les Institutions de la France sous la Révolution et l'Empire* (2d ed.; Paris, 1968), 132.

existed, such widespread insubordination would have been unthinkable before 1789.

The federation movement, which swept France during the first half of 1790 and which culminated in the great celebration in Paris on July 14 of that year, greatly increased contacts between soldiers and civilians, since many regular troops participated in these fraternal observances alongside National Guardsmen and simple citizens. Subsequently this fraternization was blamed for new outbreaks of insubordination. General de Bouillé claimed that "this federation [of July 14] poisoned the troops; soldiers brought back from the capital all the seeds of corruption; they spread them throughout the army which was, a fortnight or a month later, in full and terrible insurrection."¹¹ Such a conspiracy thesis, which conveniently identifies a handful of Parisian radicals as being responsible for a mass movement, is too simplistic and ignores or minimizes the numerous cases of insubordination before July. It does, however, point out the importance of the role of civilians in encouraging military disorders.

A decree of the National Assembly passed on August 6 was equally influential in encouraging insubordination. This decree, sanctioned by the king on August 8, established a procedure for the verification of regimental accounts.¹² The misuse of unit funds by officers had long been suspected by the soldiers in many units; this suspicion had recently resulted in the seizure of these funds by the troops of the infantry regiments of Poitou, Salm-Salm, Picardie, and Forez and the cavalry regiment of La Reine. The Assembly's decision seemed to legitimize those seizures which had already taken place and to condone subsequent activity of this kind. Before the month ended, a dispute between soldiers and officers in the garrison of Nancy over the question of regimental funds resulted in a mutiny that nearly developed into civil war.

The Nancy mutiny, one of the very few instances of violent conflict that is generally noted in histories covering 1790, developed out of tensions between the officers and men of the three regiments stationed there—the Châteaueux Swiss Infantry, Du Roi Infantry, and Mestre de Camp Général Cavalry. Friction between the soldiers and their superiors had increased through the summer of 1790 and in early August reached a climax over the issue of unit funds. The resulting insubordination created a serious problem, but one not totally dissimilar to previous incidents of this sort. The involvement of civilians, from local inhabitants to deputies of the

For contemporary justifications of the soldiers' conduct, see "Exposé justificatif de la conduite du Régiment de Touraine, depuis le 19 Mai jusqu'au 11 Juin 1790" and "Discours prononcé à l'Assemblée Nationale par M. Siau, Médecin, Député de la Garde Nationale de Perpignan, dans la Séance du 26 Juin 1790" in vols. 831 and 874 of the Maclure Collection of French Revolutionary Materials (hereafter MC), University of Pennsylvania Library.

¹¹ François-Claude Amour de Bouillé, *Mémoires du Marquis de Bouillé* (Paris, 1821), 126-27.

¹² A copy of this decree can be found in the Archives Départementales (AD) of the Bas-Rhin, 1L1439.

Assembly, was in large part responsible for escalating the mutiny to a crisis of national proportions.¹³

In April representatives from the three regiments had made a pact of federation with local citizens, including members of the National Guard. Some soldiers of the Du Roi Infantry subsequently organized a "committee," which apparently established ties with the Jacobin Club in Nancy. Thereafter confrontations between supporters and opponents of the Revolution became common. Young noble officers of the Du Roi insulted National Guardsmen and instigated brawls among the troops by paying some soldiers to antagonize those associated with prorevolutionary groups. Members of the soldiers' committee were singled out for punishment and dishonorable discharge. Protests in favor of the victims led to further penalties and further recriminations. The municipal officers, interested above all in maintaining order, attempted to mediate but generally favored the officers' attempts to keep discipline. Many ordinary citizens and elements of the National Guard, on the other hand, took up the cause of the soldiers.

On August 9, only a few days after the Assembly's decree on the verification of unit accounts, a deputation of soldiers from the Du Roi demanded an audit of regimental funds and put some officers in confinement until a sum of 150,000 livres, for which the soldiers gave a receipt, was delivered to them the next day. This success encouraged the men of the other regiments, and on August 10 two deputies from the Swiss soldiers of Châteaueux made similar demands on their officers. For their pains their officers sentenced these two—who were not even tried—to be beaten publicly with musket straps. During the punishment of the two Swiss, on August 11, a hostile crowd jeered the officers and threw stones at them. Subsequently men of the two French regiments forced the release of these unfortunates and gave them sanctuary, one in the Du Roi Infantry and the other with the Mestre de Camp Général Cavalry. The garrison commander, General de Noue, then made a public statement upbraiding the "brigands" who had perpetrated this offense against discipline. By August 15 the soldiers of all three regiments had rallied to the cause of their comrades against the officers' "tyranny," forced an apology from de Noue, exacted indemnities for the soldiers of the Châteaueux and Mestre de Camp Général, and sent a deputation of eight soldiers to present their case to the National Assembly. But in the meantime, municipal and departmental authorities appealed to the Assembly to restore order, and officers of the garrison sent their version of the affair to the minister of war.

National leaders, including the minister of war, Lafayette, the commandant of the National Guard, and influential deputies in the Assembly,

¹³ The following account of the Nancy mutiny is based upon Georges Bourdeau, "L'Affaire de Nancy—31 août 1790," *Annales de l'Est*, 12 (1898): 280-92; "Rapport de Mm. Duveyrier et B. C. Cahier, Commissaires nommés par le Roi, pour l'exécution des décrets de l'Assemblée Nationale, relatifs aux troubles de Nanci," dated Oct. 14, 1790, MC, vol. 831, and the yet unpublished work of Professor William Baldwin, which he so generously made available to me.

decided to make an example of the undisciplined troops of Nancy. On August 16 the Assembly ordered the garrison to cease all acts of insubordination within twenty-four hours and warned that failure to comply would entail the most severe punishments. General de Bouillé was appointed to implement the government's decision. In fact, order had been re-established in Nancy since the day before and was to continue until August 25, when a new instance of what the soldiers considered mistreatment by officers provoked the final insurrection.

On August 24 General Malseigne arrived in Nancy to inspect the financial records of the garrison. The next day he refused to verify the accounts of the Swiss regiment and publicly insulted the soldiers for their previous conduct. In retaliation the Swiss soldiers surrounded Malseigne's quarters and confined him there. The situation remained tense but not critical over the next three days. The two French regiments refrained from interfering. The Swiss were ordered to leave Nancy but they refused. Meanwhile, National Guard units throughout the department of the Meurthe began arriving in the city. They had been ordered to Nancy by the departmental directory, which, in turn, had been instructed to take this step by an aide-de-camp of Lafayette. Many of the newly arrived guardsmen, however, began to fraternize with the soldiers and the faction of the Nancy National Guard that supported the soldiers.

The crisis that led directly to armed conflict began on August 28. Around noon on that day Malseigne escaped and fled to Lunéville where he hoped to rally troops stationed there against the Nancy garrison. For those at Nancy, Malseigne's actions confirmed existing rumors that Malseigne and Bouillé were plotting a counterrevolutionary blow against them and local patriots. The soldiers took arms and provided weapons to sympathetic elements in the population, later described by hostile departmental officials as "the indigent and misled class of the National Guard of the city."¹⁴ The troops also arrested or otherwise confined most of their officers. Finally, they dispatched a contingent drawn from the three regiments and the National Guard to bring Malseigne back.

Meanwhile, after some hesitation the carabinier detachment at Lunéville, to which Malseigne had fled, decided to return the general to Nancy, a decision endorsed by municipal authorities of Lunéville. By August 30 Malseigne had been returned and was incarcerated at Nancy. But now a much greater danger threatened: General Bouillé was marching on Nancy with a force of four to five thousand men and eight cannon.

Upon his arrival before the city, Bouillé received a deputation from the soldiers and civil officials to whom he offered terms of complete surrender. The rebels were to lay down their arms, release their prisoners, and hand over four of the most mutinous soldiers in each regiment for trial before

¹⁴ "Copie de la Lettre écrite à MM. les Députés du Département de la Meurthe, à l'Assemblée Nationale, par les Membres du Directoire de ce Département," MC, vol. 831.

the National Assembly. If these terms were not accepted in two hours, Bouillé would attack the city and execute anyone found with weapons in hand. The soldiers, impressed by Bouillé's commission from the Assembly and overestimating his force, decided to capitulate, much to the relief of the local officials. Around 4:30 P.M. on August 31, as the Regiment of Châteaueux was leaving by one of the city gates, however, firing broke out between the Swiss and part of Bouillé's army. Although the men of the other two regiments of the garrison did not participate in the conflict, a number of civilians—later described as "outsiders and strangers" and as "the lowest class of the inhabitants of Nancy"—supported the Swiss by sniping at Bouillé's troops from buildings near the Stainville and Stanislas gates. After approximately three hours the fighting ceased. Bouillé's casualties amounted to around two hundred and the rebels lost well over fifty.¹⁵ By four o'clock the next morning all three mutinous regiments had evacuated the city.

The retribution following the uprising was severe. In the Regiment of Châteaueux twenty-three men were condemned to death, forty-one to the galleys for thirty years, and seventy-one others to be punished within the regiment. Hundreds of other soldiers and civilians were imprisoned to await punishment. The two French regiments were formally disbanded; the Jacobin Club of Nancy was closed; the wearing of the national cockade was outlawed; and the National Guard was suspended from service. Such repression smacked of counterrevolution, and critics of the government were quick to attack the handling of the mutiny. In early September a demonstration outside the National Assembly protested the "massacre of patriots" at Nancy, which many believed had been provoked by noble officers. The Jacobin Clubs in Paris and in the provinces took the same position.¹⁶ On December 18, 1790, Jean-Paul Marat continued to warn the readers of *L'Ami du Peuple* to "remember the massacre of Nancy."¹⁷

These examples of military insubordination (and many others that could also be cited) clearly indicate the intense hostility and resentment felt by the soldiers of the Royal Army toward their superiors. The sources of these antagonisms lay in prerevolutionary conditions, since the Revolution had, by 1790, altered no fundamental relationships within the army. Furthermore, the issues involved—the legitimacy of certain orders, disagreements between officers and men over fraternizing with civilians, excessive punishments, interregimental rivalries, misuse of unit funds—were, in general, of a strictly military nature and had existed long before 1789. It was only since the revolutionary events of that year, however, that the soldiers felt justified in challenging the authority of their commanders. The role of civilians was of paramount importance in this change of attitude.

¹⁵ Quotations from "Réponse au Rapport des MM. les Commissaires du Roi sur les troubles de Nancy," a document from the directory of the Meurthe, AN, F7368212; see also Chilly, *La Tour du Pin*, 241–44.

¹⁶ Poisson, *Armée et Garde Nationale*, 1: 264–65.

¹⁷ Quoted in Marat: *Textes Choisis*, ed. Michel Vovelle (Paris, 1963), 183.



Fig. 3. Jean-Jacques François Le Barbier, *The Heroism of Young Desilles at Nancy*, August 1790. This romanticized version of a young officer's action during the fighting of August 31 could hardly have provided edification for the inhabitants of Nancy who had been involved in the violence. From the Musée de Carnavalet. Photograph courtesy Bulloz.

Civilian intervention in military conflicts often had the effect of intensifying the crisis. It was particularly difficult for officers, who already were resented or detested by their men, to restore discipline when the soldiers felt that they had civilian approval for their conduct. The reaction of local authorities, especially the National Guard, was crucially important since support from these sources provided the soldiers with legitimization for their conduct that could be used to counter their officers' authority. The frequent association between soldiers and citizens who claimed to be and were identified as patriots or partisans of the Revolution provided a further, ideological justification for insubordination against officers, nine-tenths of whom were nobles and most of whom were at least mildly opposed to the Revolution.

The participation of civilians in military disorders compounded the difficulties of authorities wishing to maintain order in other ways as well. Besides providing encouragement or sanction to undisciplined troops, civil-

ians who took part in such conflicts expanded the scope of these incidents and made the restoration of order more difficult. The adherence of citizens to the cause of insubordinate soldiers increased the problem simply by increasing the number of people involved, thus making repression a more difficult task and satisfaction a more complex one. The civilians who took the side of undisciplined soldiers seldom did so solely on the basis of the specific military problems in question. More often they supported the soldiers because of a shared sense of injustice, out of a common hostility to the privileged status represented by the officers, from a desire for reform in all institutions including the army, and because of a similar social background strengthened by close personal contacts between townspeople and garrison troops. The addition of such issues, often endorsed by the soldiers as well, greatly complicated the resolution of the original problem. Furthermore, the intervention of civilians could completely paralyze the police powers of authorities. Since the line army regularly performed police functions in the eighteenth century, insubordination or mutiny automatically deprived authorities of their primary police agency. The participation of civilians, especially in large numbers, further decreased the chances that remaining police forces could or would repress their fellow citizens. When members of the National Guard, the only alternative to the line army as a police force capable of handling large-scale disorders, supported mutinous regulars, all hope of easy solution disappeared; only the bitter and costly violence exhibited at Nancy could resolve the issue.

Finally, civilian participation in military conflicts often created or exacerbated divisions among civilians themselves. The general population of France in 1790 was no more united than the army that defended it. In none of the incidents I have examined was the civilian populace unanimous in its support of insubordinate soldiers or in its hostility toward their officers. Again, Nancy provides a striking example. Unlike the National Guard, the municipal and departmental officials at Nancy had never supported the mutinous soldiers. Through most of the disorders the municipal government had simply tried to avoid a confrontation; on at least three different occasions during the first two weeks of August, for example, it refused the commandant's requests to declare martial law. The departmental officials, who played a remarkably passive role during the crisis, were extremely outspoken in their condemnation of the mutiny and in their defense of their own actions—or lack thereof—after August 31.¹⁸ Both municipal and departmental officers showed unusual vigor in the repression that followed Bouillé's victory. Even when allowance is made for duplicity, there can be little doubt that these officials disagreed fundamentally with the general

¹⁸ Bourdeau, "L'Affaire de Nancy," 284–87; see "Copie de la Lettre écrite à MM. les Députés, du Département de la Meurthe, à l'Assemblée Nationale, par les Membres du Directoire de ce Département," dated Sept. 1, 1790, in MC, vol. 831, and "Réponse au Rapport des MM. les Commissaires du Roi sur les troubles de Nancy," AN, F³682¹².

attitude of the National Guard toward the cause of the soldiers. It appears, however, that even within the National Guard there were marked divisions between radicals and moderates.¹⁹ The bitter recriminations and harsh suppression which followed the mutiny indicate that, in addition to punishing undisciplined troops, the situation afforded the opportunity for settling scores among opposing civilian factions.

Despite the complexity of the divisions that emerged from incidents of military insubordination during 1790, a single issue dominated and informed all others—namely, disagreement over legitimate authority. The origin of these conflicts lay in the soldiers' defiance of their traditional superiors, but this very often involved other disputes over authority as well. Local and national officials contended with each other; the National Guard acted independently of the civil authorities; and different elements of the civilian population took opposing sides in the military quarrels. In all of these controversies the contending parties claimed legitimacy for their position. The insubordinate troops looked for justification to the legislation of the National Assembly, to the active or passive support of local officials and the National Guard, to the approval of simple citizens, or to revolutionary principles. As a result, it was frequently impossible for any authorities to establish or maintain order. In many regions of France violent conflict came to be more typical of conditions in 1790 than the great fraternal celebration in Paris on the first anniversary of the fall of the Bastille. From Flanders to Provence Frenchmen engaged in fratricidal conflict during this "peaceful" year.

ONE LONG-STANDING ANTAGONISM in certain regions of France, particularly Languedoc, that was simultaneously given a new political character and exacerbated by the Revolution was hostility between Catholics and Protestants. In April 1790 a clash took place between these rival religious groups in Uzès; the municipal authorities appealed to the minister of the interior for a battalion of regular troops because the local National Guard, itself rent by religious and political divisions, could not restore order. Even after peace was re-established it was short-lived. About half of the Catholics of the city opposed the Revolution and the benefits it brought to Protestants; and they continued to organize and arm after April. In February 1791 they attacked the Protestants and initiated a new round of violence that had to be suppressed by armed force.²⁰

A similar conflict erupted at Montauban the following month.²¹ By early

¹⁹ Before the completion of his research, Professor Baldwin indicated to me the likelihood of such divisions.

²⁰ On the disturbance of April 1790, see the letter from the municipal officers of Uzès to Saint-Priest, dated Apr. 24, 1790, in AN, F⁹41. For the disorders in February 1791 see "Récit des événemens arrivés à Uzès les 13 et 14 Février 1791, et jours suivans jusqu'au 22," MC, vol. 832.

²¹ Unless otherwise indicated, the discussion of the troubles at Montauban is based upon

1790 divisions among the populace of this city had become concentrated in two major institutions, the local National Guard and the municipal government. The first was dominated by the commercial middle class, largely Protestant and strongly attached to the Revolution; the second was composed primarily of the nobility, clergy, and the upper echelons of Old Regime administration, predominantly Catholic and increasingly hostile to the new revolutionary government at Paris. Ancient religious animosity, resentment over the selection of centrally located Cahors as the departmental capital instead of Montauban, hostility toward the religious policies of the Assembly, and social, economic, and political rivalry all tended to confirm this division. By the spring, the focal point of these differences had become the recently created and ardently royalist "corps of volunteers," which was generally regarded as a counterweight to the National Guard. The staff of the latter opposed the municipality's policy of maintaining the "volunteers" as a distinct unit and appealed to the National Assembly. The Assembly's position was recorded in its decree of April 30, which forbade transformation of the existing National Guard structure. The ambiguous wording of the decree, however, allowed the city officials to argue that they were simply augmenting, not transforming, the National Guard.

On May 10 municipal officials began to take inventories of the property of religious establishments in the city in order to implement the Assembly's decision to nationalize Church property. But crowds soon appeared and prevented the execution of this mission. The National Guard protested and accused the mayor of being in collusion with the crowds. A contingent of fifty to sixty dragoons from the National Guard was sent to the city hall. Meanwhile, the supporters of the municipality began to arm and assemble in the courtyard of the city hall. Fighting soon broke out; five guardsmen were killed and sixteen others wounded. The dragoon detachment, including the sixteen wounded, was arrested and escorted to prison by soldiers of the regiment of Languedoc Infantry quartered in Montauban.²² The opponents of the National Guard suffered about thirty casualties. The dragoon "patriots" were mostly Protestants; their opponents were almost exclusively Catholics.

After May 10 tensions continued to mount. Homes of "patriots" were searched and weapons confiscated. Hundreds of Montalbanais bourgeois began to flee the city. In Bordeaux the municipal government organized fifteen hundred volunteers from its National Guard and on May 18 sent them to Montauban to free their imprisoned comrades. In addition, the National Assembly and most of the local officials in Quercy opposed the

Daniel Ligou, *Montauban à la fin de l'ancien régime et aux débuts de la Révolution, 1787-1794* (Paris, 1958), 210-48.

²² An explanation and justification of the conduct of the regulars during this crisis can be found in "Rapport relatif à une Pétition du Régiment de Languedoc, fait à l'Assemblée Nationale par M. De Broglie, Député de Colmar," MC, vol. 831.



Fig. 4. Prieur, *Massacre of the Patriots at Montauban*, May 10, 1790. Ancient religious hostilities in Languedoc received new directions and forms in the atmosphere created by the Revolution. Photograph courtesy Bulloz.

action of the Montauban officials. Thus isolated, the municipality accepted the mediation of a representative from Paris, Mathieu-Dumas, and released the prisoners on May 29. This isolation was compounded in June and July when district and departmental elections resulted in victory for supporters of the Revolution hostile to the municipal officers of Montauban. Meanwhile, the National Assembly began hearings on the situation in the divided city and on July 26 suspended the municipality and ordered the department to name commissioners to administer Montauban. Nevertheless, the fundamental hostilities among the inhabitants remained and flared up in minor disturbances throughout the remainder of the year.

Probably the most bloody and most thoroughly studied of the religious-political conflicts of 1790 occurred in Nîmes in May and June. James N. Hood has devoted a recent article to this problem and has provided an analysis of the background to it that may well be applicable to other similar incidents in Languedoc as well—although he too persists in referring to

1790 as "a year of social calm in most of France."²³ Since the sixteenth century the Catholics and Calvinists of Nîmes and the surrounding area had engaged in reciprocal murder, arson, pillage, and persecution which had, by the late eighteenth century, established a firm tradition of mutual fear and intolerance. Social and economic conditions reinforced such sentiments; the many Protestant merchants of the city were deeply mistrusted and resented by the Catholic peasantry of the area for their economic power as well as for their religious beliefs. Finally, as the Revolution eliminated the remaining restrictions on Protestants in France, the political power of the "almost exclusively Catholic" oligarchy of the city was threatened; and the Catholic officials and their supporters developed an increasing dislike for the Revolution and all its works. Departmental elections scheduled for late May and early June 1790 created the distinct possibility that prorevolutionary Protestants would gain office. If this occurred, many Catholics, nursed on the local traditions of intolerance, were convinced that a general persecution of Catholics, probably including massacres, would soon follow.

As in Montauban, the allegiance of opposing factions had come to be represented by the municipality and the National Guard of Nîmes.²⁴ Following the municipal elections of February the city government was firmly controlled by Catholics. The National Guard, on the other hand, was dominated by the Protestant "patriot" element of the city. The parallel with Montauban went even further. To counter Protestant control of the National Guard, a number of new companies, composed largely of poor and unemployed peasants and urban workers, had been formed by the Catholic royalist leader, François Froment. The officers of the National Guard, supported by the local Jacobin Club, demanded the dissolution of Froment's companies. Upon the refusal of the municipal officers in mid-April, the National Guard appealed to the National Assembly to endorse its position.

Thus, by the spring of 1790 the atmosphere in Nîmes was marked by a resurgence of religious prejudice, compounded by economic and social resentment and now translated into new political hostilities. Violent conflict became inevitable.²⁵ On Sunday, May 2, a demonstration by Froment's

²³ James N. Hood, "Protestant-Catholic Relations and the Roots of the First Popular Counter-revolutionary Movement in France," *Journal of Modern History*, 43 (1971): 245-75.

²⁴ For further details consult Jean Sentou, "Révolution et Contre-Révolution," in Philippe Wolff, ed., *Histoire du Languedoc* (Toulouse, 1967), 457-60.

²⁵ The following discussion of the disorders in Nîmes is based upon "Rapport de l'affaire de Nîmes fait à l'Assemblée Nationale, au nom des comités des rapports et des recherches, par Charles-Jean-Marie Alquier, Député du Département de la Charente inférieure: Le 19 Février 1791" and "Mémoire envoyé par le Régiment de Guyenne à l'Assemblée Nationale, Relatif à la conduite dudit Régiment d'Infanterie, en garnison à Nîmes, depuis le 13 jusqu'au 17 Juin inclus 1790, espace de temps qu'ont duré les troubles dans cette Ville," MC, vols. 832, 831. The latter document was certified by the colonel and forty-nine other officers of the regiment, six representatives of the departmental administration, and four commanders in the Nîmes National Guard.

units proudly paraded the white cockade of the royal house and chanted, "Long live the King; long live the Cross; down with the Nation!" Later the same day, around 5:00 P.M., a group of noncommissioned officers and soldiers of the regiment of Guyenne Infantry, which had been stationed in the city for nearly five years, tore the white cockades from the clothing of some demonstrators. A crowd quickly gathered, pelted the troops with rocks, and forced them to barricade themselves in a nearby house. The lieutenant colonel commanding the regiment assembled the unit and escorted his beleaguered men safely back to the barracks. The next day anti-revolutionary activity recommenced, but this time it was directed against Protestants and the men of the Guyenne Infantry as well. The crowd again stoned a number of soldiers; one infantryman was wounded by a saber and another shot to death. The municipal government, dominated by Catholics, took no action whatsoever to restore order. By 7:00 P.M. the frustrated soldiers, who had already seen a number of their comrades hurt—one fatally—attacked their tormentors and dispersed the crowd on their own authority. At the first order from their officers, however, the troops obediently returned to their quarters. An uneasy peace settled on Nîmes.

In six weeks the peace was broken by a veritable civil war within the city. During the evening of June 13, another Sunday—one would think a particularly inappropriate day for Christians to slaughter each other!—fighting began between the armed forces of the royalist Catholic and patriot Protestant elements in Nîmes. Other citizens joined in as muskets, swords, pitchforks, and rocks were used as weapons for the combat. The Guyenne Infantry was called out and put in formation at the request of a royal commissioner and one city official; but, except for isolated soldiers or groups of soldiers, the troops did not engage in combat. The regiment did provide patrols to police the city through the tense evening. Meanwhile, reinforcements to both factions poured into the city from the countryside during the night, and fighting resumed the next day. The struggle took on the characteristics of guerrilla warfare as snipers fired from buildings. One building used for such purposes was a Capuchin convent that was subsequently captured and sacked while five monks and three civilians found in it were killed. Surrender gave no guarantee of safety; prisoners were slaughtered out of hand. Cannons were used by some of the Protestant units of the National Guard against their Catholic opponents. The men of the Guyenne Infantry stood by in battle formation at their barracks but were not employed by the civil authorities to halt the disorders. Fighting and atrocities continued on June 15 and 16 and ceased only on June 17, when three thousand National Guardsmen arrived from Montpellier. By then more than three hundred persons had been killed and 120 houses had been destroyed in Nîmes and its immediate vicinity.

As in the case of the military disorders during 1790, these conflicts included a number of issues. Fundamental was the ancient religious animosity

that persisted in this region, animosity reinforced by social and economic distinctions. The Revolution added a new and extremely volatile political dimension to these differences by its partial destruction of old authority and its as yet incomplete introduction of new authority. Indeed, the dispute over authority was central to all other questions. In Montauban and Nîmes the power of local officials was seriously undermined by the fact that the National Guard challenged their authority. In order to provide themselves with reliable forces to maintain order, the municipal officers sponsored alternate formations within the National Guard. The existence of rival police agencies, in turn, made law enforcement a partisan affair and readily led to a complete breakdown of law and order. To establish the legitimacy of their position the older National Guard units sought the support of the National Assembly; in a less formal and less explicit way the municipalities appealed to the traditional authority of the king. These institutions of central government, however, faced the same problem as did the local officials: they could not command unquestioned obedience. Regular army units stationed in these two cities, the Regiment of Languedoc at Montauban and the Regiment of Guyenne at Nîmes, were not employed to establish order because of doubts about their discipline during this period of military disintegration and because a number of soldiers, particularly in the Guyenne Infantry, had displayed sympathy for one of the rival factions.²⁶ Again, violence appeared to be the only means of resolving the problem of authority; and probably the most important heritage of this violence was, as Hood has demonstrated in the case of Nîmes, the growth of a popular counterrevolutionary movement that was to try French governments throughout the Revolution.

OTHER CONFLICTS DURING 1790 also arose out of pre-existing problems, complicated by the Revolution and the policies established in its name. The attempt of the National Assembly to distinguish between the personal and property rights connected with feudalism and to require redemption payments for the latter after the formal abolition of the feudal regime was never accepted by the peasantry of France. The year 1790 saw a continuation of peasant rebellions against feudal payments, which were to last until the institution was completely and definitively abolished on July 17, 1793. In January 1790 new *jacqueries* against feudal obligations began in Quercy and Périgord; subsequently they swept through upper Brittany; in May similar uprisings took place in Bourbonnais and nearby regions; in the autumn the peasants of Gâtinais rebelled against the payment of redemp-

²⁶The soldiers of the Languedoc had made a "pact of federation" with the National Guard of Montauban and Toulouse in April 1790; see Albert Soboul, *Les soldats de l'an II* (Paris, 1959), 54. On the soldiers of Guyenne, see "Rapport de l'affaire de Nîmes," MC, vol. 832.

tion fees; and by the end of the year there was another series of disorders in Quercy and Périgord.²⁷

These disturbances have not received the same intensive study as the uprisings of the previous year, which led directly to the antifeudal legislation of August 4, and the present study cannot aspire to fill that lacuna. It is, however, both possible and useful to investigate one example of this persistent problem, namely the disturbances in Quercy during December 1790 and January 1791.²⁸ Feudalism had been particularly onerous in Quercy prior to the Revolution, and the peasants were deeply disappointed at the retention of many payments after its apparent abolition. In May, June, and July of 1790 there had been attacks on seigneurial property in the province, now the department of the Lot. These attacks were coupled with refusals to pay the redemption fees; and as a gauge of their sincerity, peasants in a number of localities in the Lot, such as Thuron and Gindon, set up gallows on which they promised to hang anyone who made such payments. The authorities, wishing to enforce the law but fearing general insurrection, were paralyzed. But some feudal lords, sure of their legal position, used force to exact their dues. Once more, the breakdown of a consensus about lawful authority led to attempts by contending groups to support their position by violence.

On December 3 a crowd of armed peasants, estimated at five thousand persons, attacked the district capital of Gourdon where local officials had assembled a hundred line troops and two squads of the *maréchaussée* to enforce the law. In the face of such odds, the troops had to retire while the crowd pillaged certain sections of the town, particularly the houses of the rich, and ravaged outlying areas. Departmental authorities summoned more regular detachments to the district from Montauban and Figeac. A detachment of the regiment of Languedoc Infantry in Gourdon attempted to collaborate with the Gourdon National Guard to restore peace. The guardsmen, however, soon joined the armed peasants, and the regulars were besieged in a church where they had taken refuge. Only after protracted parleying did the peasants allow the soldiers to depart the town unharmed. Pillaging continued in Gourdon until December 7.

Troubles persisted in the department through the rest of the month. Many nobles had begun to organize and arm to protect themselves and their property. One such group assembled at Lauserte on December 17 after four or five chateaux in the neighborhood had been pillaged. The next day another band of nobles who had gathered at Montcuq opened fire on a crowd and touched off a riot that had to be repressed by over a hundred line

²⁷ Lefebvre, *French Revolution*, 141.

²⁸ The following account is based upon "Rapport des Messieurs J. Godard et J. Robin, Commissaires civils, Envoyés par le Roi, dans le Département du Lot, en execution du Décret de l'Assemblée Nationale, du 13 Décembre 1790," MC, vol. 832.

and National Guard troops. Such action by the nobles gave credence to rumors that had been circulating in the area about a counterrevolutionary plot. In turn, more peasants armed themselves and committed violence against their former lords. Between December 17 and January 18 approximately thirty more chateaux and houses in the district of Lauserte were burned and sacked. Unrest and disorders broke out elsewhere in the Lot as tensions further increased. By early February the only solution to the situation that could be offered by royal commissioners sent to investigate the problem was additional reinforcements of line troops. Here, as elsewhere in France, such sporadic violence would continue for the next two and one-half years, until the abolition of the last vestiges of feudalism.

The antifeudal disturbances in the Lot provide a vivid example of how complete the collapse of law and order could become. The ambivalent position of the National Assembly had laid the bases for such conflict in August 1789 when it simultaneously declared the feudal regime abolished in its entirety and qualified the abolition by exceptions and limitations. The peasants acknowledged only the total destruction of this hated institution; the possessors of seigneurial rights insisted on the more precise legal distinctions. The ambivalence of the situation allowed law enforcement agencies to choose their allegiance. The National Guard, recruited locally and sympathetic to the peasants, often simply stood by or even aided the peasant violence. The regular troops attempted to enforce the decisions of the central government but were overwhelmed by the numbers of their opponents. The nobles, feeling themselves denied the protection of the law, formed their own armed forces. Since the two legally constituted police agencies, the National Guard and the Royal Army, were in conflict and since hostile civilian factions had taken up arms to defend their interests, the issue was decided by superior force. Regardless of the decisions of lawmakers in Paris, it was the action of local peasants that determined the fate of feudal payments.

While the incidents thus far discussed do not present a complete picture of the extent or variety of violence during 1790, they do demonstrate how hostilities dating well back into the Old Regime found full expression in the circumstances and atmosphere created by the Revolution. One common characteristic of these conflicts, whatever their bases, was that they were commonly posed in political terms. Despite the multiplicity and complexity of the issues involved, the fundamental question was: who represented legitimate political authority? The Revolution had raised this question but had not provided a generally acceptable response to it. The answer was only gradually—and even then incompletely—worked out through a series of struggles for political power. The remainder of this study will concern itself with a few striking and representative examples of violent conflicts that were explicitly political in nature.

DURING 1790 POLITICAL ACTIVITY of any kind, even the most normal and legitimate, could potentially result in violence. As departmental elections had directly contributed to the hostilities at Nîmes, local elections elsewhere frequently resulted in disorders. For example, in early February elections in the district of Aix-en-Provence led to conflicts between opposing political factions that required the intervention of regular army troops.²⁹ At Hagenau in Alsace rival political groups—identified as the *Droite* or Patriots and the *Gauche* or *Links*—had developed over the issue of recently elected municipal officers.³⁰ The National Guard, as happened with alarming frequency in this period, was similarly divided, with the most recently formed units giving their allegiance to the new municipal government and the older units opposing it. On July 24 fighting broke out between the rival groups in the National Guard. Before the regiment of Picardie Chasseurs, which was called in by the town officials, could restore order, four people had been killed. Subsequently the officials used this cavalry unit to disarm the older National Guard elements in opposition to them. During the last week of October a popular uprising at Issoudun in Berry forced municipal officials to flee the city. Only after the National Guard, local *maréchaussée* forces, and detachments of the Royal Roussillon Infantry and Royal Piémont Cavalry had restored order could these officials resume their posts in early November.³¹

Sometimes political conflicts were more ill defined than struggles between groups contending for local office. Such controversies centered on the elementary but amorphous question of support for or opposition to the Revolution. Virtually all conflicts during this tense period had political undertones that were all too readily expressed in violence. The execution of one Anicet Martel, convicted of the assassination of M. d'Albertas, former president of the Estates of Provence, in Aix on August 2, 1790, became the occasion of a political riot. Martel, a member of the National Guard of Toulon, had apparently become something of a symbol of opposition to the aristocracy, and the occasion of his execution touched off a riot in which Martel nearly escaped. The intervention of a contingent from the Lyonnais Infantry allowed the sentence to be carried out but created hostility toward some of the officers commanding the detachment. This disturbance quickly turned into a demonstration against aristocrats and opponents of the Revolution in general. Two days and three hundred regular troops were required to restore order.³² Friction between units

²⁹ Carnot, "Régiment de Lyonnais," 9.

³⁰ For a description of this controversy, see AN, F736861. The documentation, although it uses the modern political terms "Right" and "Left"—in both French and German—does not, unfortunately, make clear what political positions were embraced by these opposing factions. It seems that the "Left" may have been the more conservative of the two.

³¹ AN, H 1453.

³² Carnot, "Régiment de Lyonnais," 16–17.

garrisoning Belfort and the civilian populace resulted in political violence in that frontier town in October. The regiments of the Royal Liégeois Infantry and the Lauzun Hussars had been sent to Belfort shortly after both units had contributed to the repression of the Nancy mutiny. Their stay in Belfort had been marked by a number of incidents in which local citizens had insulted and abused these soldiers for their role in the "massacre of patriots" at Nancy. On October 21, following a dinner given by the infantry officers for their colleagues in the hussar regiment, a crowd of officers and soldiers had paraded through Belfort with white handkerchiefs at the ends of their drawn swords, crying: "Long live the King! Long live aristocrats! To the devil with the nation!" Objections to such sentiments by local citizens were met by insults, mistreatment, and blows with the flat of their sabers. De Guy, the garrison commandant, finally succeeded in quieting things down by ordering all troops to their barracks. Shortly thereafter the arrest of the most culpable officers and the transfer of the two regiments were ordered by the National Assembly.³³

Political rivalry in 1790 entailed much more than the simple possession of office; it also included a struggle for political legitimacy, which would—it seemed to many—determine the direction, the extent, and even the success or failure of the Revolution. Given the great stakes involved, the divisions were often ideological and the conflicts, necessarily, intense. In light of the temporary weakness of the central government and its own inability to establish any kind of a political consensus, the issues were frequently resolved by local violence. In these struggles, control over organized armed forces was critically important. Local National Guard units, however, were often as bitterly divided as the civilian groups vying for authority; consequently, their participation escalated, rather than repressed, the violence. When regular troops were politically motivated, as were the Royal Liégeois Infantry and Lauzun Hussars at Belfort, they too aggravated conflicts. But even the apparently apolitical intervention of well-disciplined line units, as at Hagenau, Issoudun, and Aix, had decisive political consequences by securing one party or another in power. Where a peaceful recognition of legitimacy was impossible, a successful appeal to force sufficed.

THUS, THE AFTERMATH of the revolutionary events of 1789 was not, as it is usually depicted, a settling down to work out the details of the revolutionary program; it was, rather, a struggle for political power that brought in its wake the widespread, if not general, breakdown of law and order during 1790. While each of the conflicts treated in this study arose from local conditions and possessed its own peculiar characteristics, together they dis-

³³ "Extrait des procédures criminelles du Tribunal du District de Belfort," Oct. 21, 1790, AN, F945, and "Rapport fait à l'Assemblée Nationale dans la séance du samedi 30 Octobre, au nom des Comités militaires et des rapports, sur les événemens arrivés le 21 Octobre à Belfort, par Muguet, dit Nanthou, Député de la Haute-Saône," MC, vol. 832.

play certain general tendencies that account for this phenomenon. In order to provide further evidence of the severity and complexity of this problem, this article will conclude with an examination of two incidents. These examples, which occurred in Marseilles and Lyon, were selected for a number of reasons. Each took place in a comparatively large population center with a heterogeneous social composition. The issues involved, while they may not be entirely typical of conditions in the country at large, are not, on the other hand, unique. And finally, the documentation for these incidents—largely unexploited—is reasonably complete.

The inhabitants of Marseilles, like those of other cities and towns of France, had long before the Revolution objected to quartering royal troops; they resented the costs of lodging and feeding the soldiers and feared the threat that the presence of such forces constituted for municipal autonomy. The early Revolution, with its emphasis upon individual and local rights, encouraged the Marseillais to take action about this complaint.³⁴

By early 1790 the municipal officers, supported by a large segment of popular opinion, were applying continual pressure on the military commandant of the region, the marquis de Miran, to remove all regular army units from the city. Miran, however, refused to take such action without authorization from the minister of war. Furthermore, in his correspondence with the minister he warned that the withdrawal of troops would seriously alarm "all honest citizens"—that is, the wealthier elements of the city—who wanted to retain royal forces to police "the riffraff," or *canaille*. Thus within the larger struggle between local and national governments over municipal autonomy another conflict existed among the inhabitants of Marseilles.

By April 21 the Marseillais had succeeded in having all regular troops withdrawn from the city except two regiments: the Swiss regiment of Ernest, whose task was to guard the port facilities, and the Vexin Infantry, which manned the forts protecting the city—Saint-Jean, Saint-Nicolas, and Notre Dame de la Garde. The municipality, vigorously seconded by the local Jacobin Club, then demanded that the National Guard replace the Vexin Infantry as a garrison for the three forts. In the face of Miran's persistent refusal, the Marseilles National Guard simply seized control of the fortifications on April 30. Since most of the soldiers and some of the officers of the Vexin Regiment were favorably disposed toward the Marseillais, the transfer was almost without bloodshed. One officer, Major de Beausset, who had counseled resistance, was, however, lynched by a crowd that included soldiers from his own regiment.

The seizure of the three forts had directly flaunted royal authority, and Louis XVI replied to the challenge by ordering the forts returned to royal troops. Through May the municipal officials and the deputies from Mar-

³⁴ The following description of events in Marseilles is based upon documents in AN, F736591 and "Affaire du M. d'Ambert," AG, YA 447.

seilles to the National Assembly attempted to justify their position to the central government. They argued that the expense of quartering troops was an excessive burden on the city, that the National Guard could provide the same security, and that the royal forces constituted both a threat and a source of indignation to the citizens of Marseilles. Although the king did not change his position, the Marseillais proceeded to disarm Fort Notre Dame de la Garde and to demolish the other two forts, a task completed during the first half of June. Without officially revising its stand, the central government tacitly capitulated by ordering the transfer of the Vexin Infantry in October, leaving only the Swiss regiment, isolated and harassed by local civilians, in the city.

These incidents had in only a few months radically altered the political relationship between Marseilles and the royal government. By their determined action the municipal officials had forced the evacuation of almost all regular troops and replaced them with units subject to their own authority. The weakness of the central government and the divisions within the Royal Army, so graphically evidenced by Beausset's assassination, prevented any effective response to this challenge. As a result, local power increased at the expense of the central government. This autonomy, so forcefully asserted by the Marseillais, was to last for the next three years, only to disappear during the bloody federalist revolts at the hands of a very different and much more resolute central authority.

The replacement of royal troops by the National Guard also meant a shift in power within the city. The lower classes and more radical political groups, notably the Jacobin Club, had led the opposition to the garrison, and their success represented a defeat for the wealthier, more moderate elements of the city. With the displacement of royal authority, many of Miran's "honest citizens" felt that they were dominated by the *canaille* and deprived of the protection of the law. Like the rivalry between local and national authorities, this division between rich and poor in Marseilles would reappear later in the Revolution and lead to greater violence than in 1790.

Although no stranger to internal disorders—following, for example, a silkworkers' strike in 1788 and attacks upon the city tollgates in mid-1789—Lyon witnessed a marked increase in the incidence and intensity of violence in 1790. In February the royal arsenal was pillaged, and the mayor, who represented the wealthy merchants, was attacked by a crowd, including National Guardsmen, and forced to flee the city. In late June and early July crowds again attacked the tollgates, which levied taxes on products entering the city, thereby increasing their cost or discouraging their importation.³⁵ Intimidated, the municipality removed the tollgates on July 10,

³⁵ For the earlier troubles in Lyon, see the letter of Delhorme, dated Feb. 10, 1790, AN, BB³⁰⁸⁷, and Colonel H. de Buttet, "La Mission de Monsieur de la Chapelle, Maréchal de camp commandant les troupes envoyés à Lyon pour le rétablissement des barrières et ses rapports

but the National Assembly ordered them restored only three days later. Riots again broke out, and by the end of the month martial law was declared and an army of forty-six to forty-eight hundred regulars under General de la Chapelle was ordered to Lyon and the surrounding region.

While this measure restored order, it also created new problems. Many civilians objected to the financial burden of supporting so many troops and resented the off-duty work by soldiers, which intensified competition for jobs. The officers felt that quartering their troops among the civil populace, particularly the turbulent Lyonnais workers, posed a danger to discipline.³⁶ Local supporters of the Revolution were alarmed by the counterrevolutionary plots—both real and rumored—which centered on Lyon and feared that the noble officers commanding the troops in this area were party to them.³⁷ Finally, many local citizens, especially in the National Guard, resented the royal troops who were usurping the functions of the Guard. Inevitably these tensions, fears, and suspicions led to conflicts over authority and the maintenance of order.

In early October the municipal government demanded that La Chapelle provide more weapons to the National Guard from the royal arsenal in Lyon. The general refused to do so without orders from the minister of war or the king. The civil officials replied that the arsenal was now a “national depot,” which they could use as conditions required. La Chapelle was adamant, the officials persisted, and the issue remained unresolved. Later the same month the National Guard and what La Chapelle called “the lowest class of the people” requested the removal of foreign troops from the city and their replacement by French regiments. In early November the Jacobin Club demanded that all royal troops be withdrawn from Lyon and the arsenal be handed over to civil authorities. La Chapelle rejected all these demands and like Miran, his counterpart at Marseilles earlier that year, was convinced that “if the line troops departed . . . the well-to-do people would leave the city along with most honest citizens and even most of the municipal officers.”³⁸

By November new municipal elections were approaching, and political agitation was at a fever pitch. The Jacobins accused La Chapelle of planning to deliver Lyon to counterrevolutionaries, of wanting to massacre all patri-

avec la municipalité (Septembre 1790 à janvier 1791),” in *Actes du quatre-vingt-neuvième congrès national des sociétés savantes: Lyon, 1964*, Section d’histoire moderne et contemporaine, bk. 2 (Paris, 1965), 1: 220. For events in 1790 see “Lyon 1790,” AG, YA 447. This source provides the basis for the following discussion of events in Lyon, unless otherwise noted. It should be pointed out that Colonel de Buttet used the same source. Because of the different purposes of M. de Buttet and myself, it was preferable to return to the original documentation rather than to base my account on Buttet’s article.

³⁶ La Chapelle expressed precisely these concerns in a memoir dated Aug. 26, 1790, in AG, YA 447.

³⁷ “Décret sur la conspiration de Lyon; Précédé du Rapport fait à l’Assemblée Nationale, au nom de son Comité des Recherches, Par Charles Voidel, Membre de ce Comité, Séance du 18 Décembre [1790] au soir,” MC, vol. 832, is but one of many examples of this concern.

³⁸ Letter from La Chapelle to the minister of war, dated Oct. 10, 1790, in YA 447.

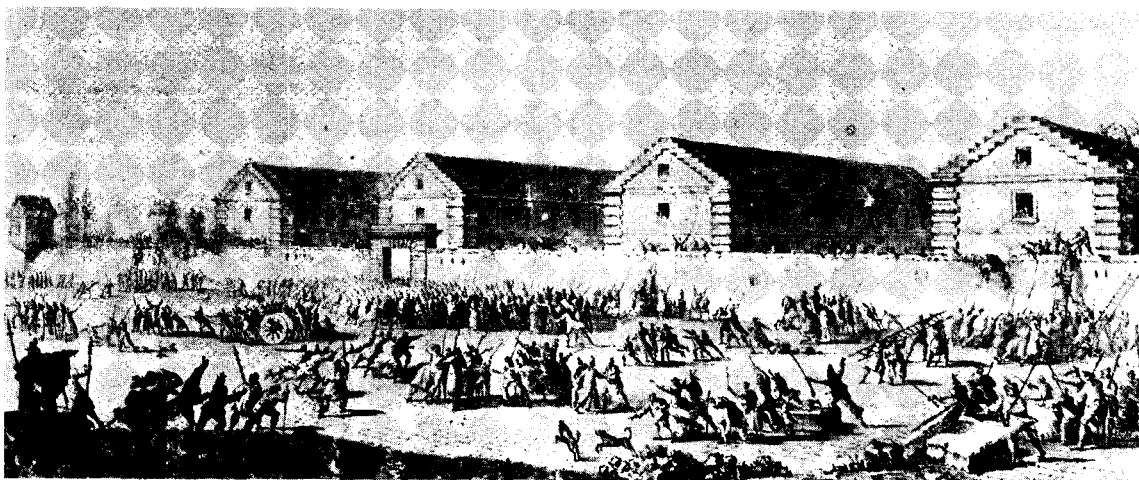


Fig. 5. Prieur, *Pillage of the Arsenal of Lyon*. The royal arsenal in this city was attacked by crowds in early July 1789 and again in February 1790 and remained a point of controversy between civil and military authorities later in that year. Photograph courtesy Bulloz.

otic citizens, of being part of a clerical plot.³⁹ Although the municipal officers expressed their confidence in the general and ascribed the slanders to “some reprobates,” they made it quite clear to La Chapelle that no regular troops were to be employed in the city except on their explicit orders. Nevertheless, La Chapelle’s position deteriorated. Not only did the Jacobin accusations encourage the gravest suspicions about his politics, especially among those who were the usual victims of repression, but he also began to lose control over his troops. He claimed that many of the soldiers in his command “completely identified themselves with the inhabitants of the lowest class.” Although discipline had not collapsed so completely in all units, La Chapelle claimed that he could depend fully on only two foreign regiments—one Swiss and one German—without which he could not answer for Lyon for more than twenty-four hours.

La Chapelle’s authority had been thoroughly undermined. In mid-December there was a request in the National Assembly that he be replaced; and on January 2, 1791, he was relieved of his command.⁴⁰ The removal of La Chapelle did little to alleviate the fundamental problems of Lyon. The quarrel over a garrison continued for another year, with the wealthy merchants and proprietors demanding it for protection against the workers and their opponents decrying its uselessness, its expense, and the economic disruption that it caused.⁴¹ The rich won their case and a garrison

³⁹ There were indeed some grounds for suspicions about La Chapelle. He did, in fact, emigrate in 1791, and by 1792 he was serving as a major general in the Army of the Princes. But more incriminating than his eventual emigration is a reference in a letter of the vicomte de Mirabeau, dated Lausanne (probably Dec. 16, 1790), which, in discussing the emigrés’ progress in Lyon, noted, “Mr. de la Ch. . . has the troops well in hand.” See AN, 119 AP.

⁴⁰ Buttet, “Mission de M. de la Chapelle,” 236.

⁴¹ On this question see the dossier marked “Affaire de Lyon, Novembre 1791” in AG, YA 447.

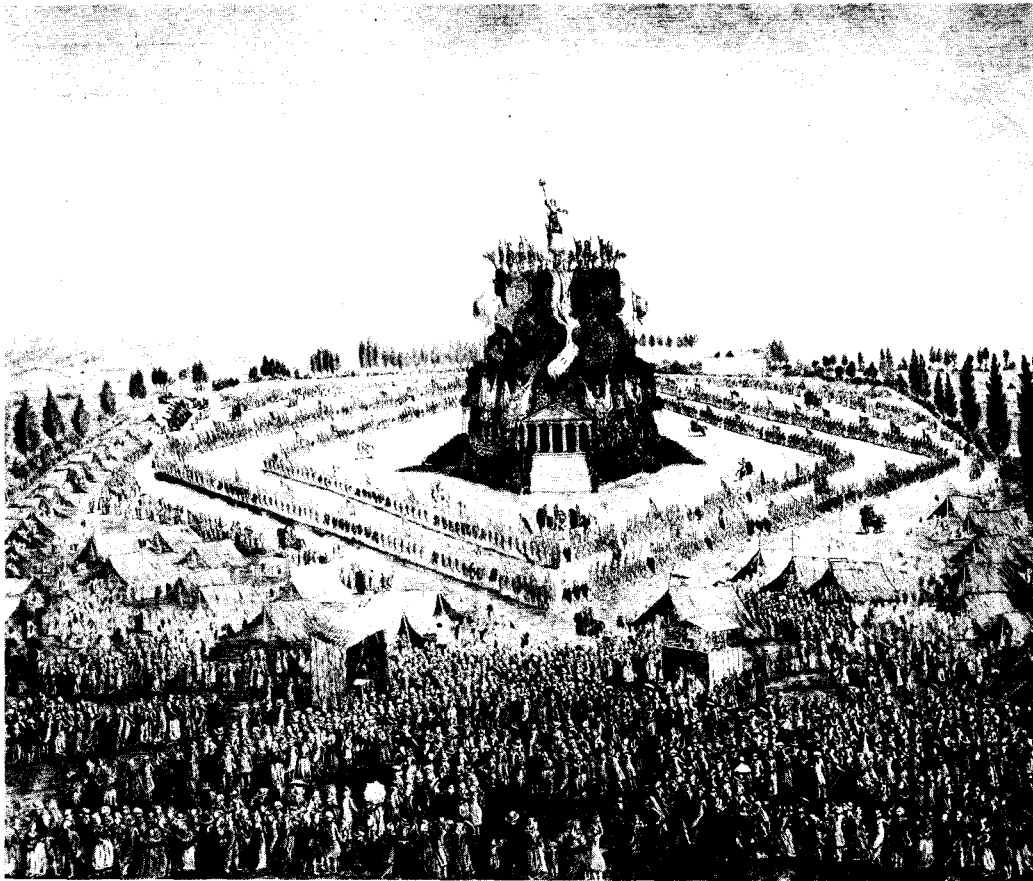


Fig. 6. *Federation Camp of Lyon, May 30, 1790.* Within a month of this fraternal celebration Lyon again became the scene of bitter violence, a condition more typical of relations within the city than the scene depicted here. Photograph courtesy Bulloz.

was kept at Lyon, but hostility toward it remained strong among the lower classes of the city.

The situation in Lyon during 1790 bears some striking similarities to that of Marseilles, yet at the same time it reflects its own particular circumstances. In both cities the municipality disputed the authority of the central government; but the officials of Lyon were less decisive and less daring in their controversies with Paris. The municipal officers of Lyon did try to usurp the authority of the minister of war by demanding control over the royal arsenal; they also insisted on their right to the final decision on the use of regular troops within the city. They were not, however, willing to push any of these claims to the point of confrontation. They would never have supported, or even condoned, the seizure of military facilities, as the Marseillais officials had done. Likewise, the Lyon authorities responded to the decision of the central government to assemble a large force of regulars around the city with passive acquiescence rather than with vigorous protest

as at Marseilles. Finally, on the inflammatory issue of the tollgates, the municipality first bowed to the action of its citizens but subsequently revised its position when the Assembly decided to restore them and provided the armed forces to implement this decision. This timorousness was due, above all, to the precarious authority of the Lyon officials.

The municipal officers of Lyon were in the unenviable position of having to govern a divided and turbulent populace without having any secure authority over the available forces of order. By its participation in the overthrow of the mayor in February, the National Guard had demonstrated that it was not merely a passive instrument of the established city government. Furthermore, by its constant support for the National Guard, the Jacobin Club had established considerable influence over this force. Because of this influence and because of their popularity among the masses of the city, the Jacobins enjoyed an authority which, if not opposed to that of the municipality, was independent of it. In Marseilles there was a much closer and more secure relationship between the municipal authorities on one hand and the National Guard, the Jacobin Club, and the masses on the other hand. This difference was a major factor behind the ambivalence of the Lyonnais municipality.

In both cities the presence of a regular garrison was a critical issue. In both cities the radical clubs and the lower classes strongly objected to the presence of line units, and in both cases the motivation was essentially the same. Besides the threat that they represented to local autonomy as agents of royal power, regular troops traditionally had been employed to support the established regime. They were feared because their discipline and professionalism made them the most effective means of repressing any challenge to established authority.⁴² In the fluid political conditions of 1790 such forces could decide local disputes simply by obeying those in power and thus thwart the ambitions of politically aware but politically impotent rivals. Beyond this, suspicions about the political loyalties of many noble officers created the fear that disciplined regulars might be used to serve the cause of counterrevolution. In addition, the presence of line troops had important economic and social implications. The wealthier upper classes of Lyon and Marseilles saw a royal garrison as the surest protection for their influence and wealth. The lower classes were alarmed that large numbers of soldiers would constitute a major burden on public funds, create unfair competition for jobs, and, as in the past, repress any form of economic protest. They preferred the replacement of regulars by the National Guard, which was more representative of the local populace and more sympathetic to their interests. When the transfer of line units could

⁴² The widespread belief that foreign regiments were more alien to French conditions and more strictly disciplined than French regulars explains the special animosity toward and fear of these units. Although possibly valid as a general judgment, there were so many exceptions to such a hypothesis in the course of the Revolution that it had little practical, predictive value.

not be obtained, the popular societies and common citizens attempted to break down the soldiers' discipline and win them to their cause—often with substantial success.

Throughout the controversy over the garrison the role of the Lyon municipality was, as we have seen, a passive one. Possibly the officials were in agreement with their wealthier constituents but dared not make this public; probably they felt that, although they could not exercise direct control over the regulars, the line units could be relied upon to defend their authority; and certainly the danger of attack by emigrés and foreigners necessitated a strong, disciplined defensive force. Regardless of these considerations, however, the Lyonnais officials had no real alternative to inaction. Unlike their counterparts at Marseilles, they were sure neither of popular support nor of the National Guard. The regular army commandant answered only to Paris. The fragmentation of authority at Lyon had become so great that the municipal officials held office, but little real power. As a result, they did nothing to resolve the problems plaguing the city. Only three years later these difficulties, the relationship between local and national sovereignty and the intense internal divisions within Lyon, resulted in civil war. The city was captured, after a two months' siege, by the armies of the central government, and its fortifications as well as all the homes of its wealthier inhabitants were ordered destroyed. Since even these drastic measures were not entirely successful, one might well pity the impotent authorities of 1790.

ALTHOUGH THE TROUBLES at Lyon, Marseilles, and elsewhere inevitably reflected peculiar local conditions, they all indicate the general character of disorder during this period. As is clear from this investigation based primarily on records dealing with the employment of regular army units as police, violent conflict was a common phenomenon in 1790. Although Paris was spared, many of the greatest cities in France—Lille, Nancy, Montauban, Nîmes, Marseilles, Lyon, and others—were the scenes of bitter struggles. Smaller cities and towns such as Perpignan, Issoudun, Hagenau, and Belfort, villages like Gindon, Montcuq, Thuron, and the countryside around Nîmes, Aix, and Lauserte, for example, also witnessed more or less serious disturbances. More methodical research in a greater number of sources would undoubtedly provide more extensive and more conclusive evidence about the prevalence of violence in France at this time.

Even without an exhaustive study, however, some general observations can be made concerning the problem of law and order during 1790. Despite the variety of issues—certainly greater than the examples cited here indicate—the most distinctive and significant feature of conflict in 1790 was its political nature. Pre-existing problems, such as discontent in the army or religious animosities, acquired a new intensity and form in the highly

politicized atmosphere created by the Revolution. Other problems arose directly out of the political questions raised by the Revolution itself: for example, the relations between local and national officials and control over armed forces. The Revolution introduced modern politics into France and the struggles for power and place among contending factions and parties that are an essential part of it. Yet, beyond this, the Revolution created conditions that readily allowed, even encouraged, the definition of localized controversies and opposing interests in political terms, regardless of the nature of the issues at stake. Even the authorities and agencies charged with the maintenance of law and order commonly viewed their functions in political terms and thus seriously undermined their own ability to accomplish their duties.

The revolutionary events of 1789 had fragmented political authority in France. As a result, groups and individuals enjoyed considerable latitude in determining for themselves where legitimate authority lay. Political consensus was destroyed and replaced by political competition. Since none of the competitors were able to provide an orderly settlement that was generally acceptable, they often sought the solution to their problems in an appeal to force. Thus the violence of 1790 may be seen as a "natural" consequence of the incomplete revolution of the previous year. Furthermore, as other research indicates, such an increase in the level of violent conflict after the original transfer of political power may well be a phenomenon common to an entire category of revolutions, or even to revolution in general.⁴³

Since the question of legitimacy was frequently settled by recourse to violence, the police agencies of the country, particularly the National Guard and the Royal Army, which together nearly monopolized organized armed force in France, occupied a crucial position. As passive tools of the established authorities, they could be used not only to enforce the law, but also by obediently carrying out the orders of those in power they could, to a large degree, confirm the legitimacy of that power. Such a simple solution, however, was not to be. Like the rest of society, these two institutions were bitterly divided. Differences of opinion and hostilities existed between and within many units of both forces, and these divisions were expressed in essentially the same terms that divided society as a whole. Regular soldiers and National Guardsmen, like their fellow citizens, questioned the legitimacy and powers of those in positions of authority. This situation both expanded the scope of conflicts among the populace and destroyed the ability of officials, very frequently political moderates who hoped to merge new and old principles and institutions, to maintain law and order during 1790 and after.

⁴³ The results of this study offer confirmation of many of the characteristics of revolution described by Rule and Tilly in "1830 and the Unnatural History of Revolution." This confirmation is particularly impressive because I was unaware of their conclusions not only while compiling my research, but even during earlier drafts of this article.

Abolitionists, Irish Immigrants, and the Dilemmas of Romantic Nationalism

GILBERT OSOFSKY[†]

NEW ENGLANDERS HAVE TRADITIONALLY prided themselves on their exclusiveness. The signs of obvious unwelcome that awaited the Quakers in the seventeenth century continued in less strident though always visible forms in the eighteenth century. No one attempted to lynch the Scotch-Irish Presbyterian immigrants who tried to land at Boston port in the 1720s, but mobs gathered to prevent their settling. The familiar arguments were made: immigrants were likely to become public charges, "foreigners" would eat Bostonians out of house and home, they represented unfair competition to the native-born working classes. Some Scotch-Irish immigrants made their way to Maine, Vermont, and New Hampshire, but the major direction of their movement shifted to the less-populated frontier sections of Pennsylvania, Virginia, and the Carolinas. The great migrations of German and Protestant Irish immigrants that settled the colonies in the eighteenth century largely passed New England by. At the opening of the nineteenth century that section and its leading city remained the most ethnically and culturally homogeneous area of the nation.¹

The migration of Roman Catholic Irish immigrants, which began in small numbers after the War of 1812 and continued in ever-increasing numbers after the 1830s, changed all that. By 1855 some fifty thousand Irish lived in the city of the Puritans.² Their presence—in Boston and throughout the nation—their "ideology," their faith, and their views on slavery and social change had a profound effect on the American political system in the antebellum years. The migration of this essentially conservative ethnic group coincided with a major era of radical social change and reform in America and presented a serious dilemma for the antislavery movement.

[†] Gilbert Osofsky died on August 26, 1974. An obituary appeared in the April 1975 issue of the *AHR*. Dr. Marion S. Miller of the University of Illinois at Chicago Circle kindly oversaw the preparation of this article for publication. Mr. Osofsky wished to acknowledge the grants from the Louis M. Rabinowitz Foundation and the American Council of Learned Societies that permitted the completion of the article and to thank Marion S. Miller and Sue Robinson of the University of Illinois at Chicago Circle for their careful readings which considerably improved its content and style.

¹ Henry Jones Ford, *The Scotch-Irish in America* (Princeton, 1915), 192, 211, 221–48.

² Oscar Handlin, *Boston's Immigrants: A Study in Acculturation* (New York, 1968), 52.

The basically Protestant, evangelical, and radical Garrisonian reformers attempted for an entire generation to reach out for Irish sympathy and support. They communicated directly with the most prominent Irish leader of the masses in the 1830s and 1840s, Daniel O'Connell; they established contact with other Irishmen abroad who were sympathetic to the antislavery movement; they appealed to the prevailing spirit of Irish nationalism; and they engaged in one of the most vigorous attacks on American nativism and Know-Nothingism from the very moment these forces emerged in the 1830s to the time of their demise in the late 1850s. The antislavery movement's attempts to reach beyond the traditional bitter hostility of Protestants toward the papacy and "Jesuitism" and to confront openly the most abrasive social questions of the time—slavery and disunion—constitute an important phase in the history of egalitarian thought in America. Antislavery faith ideally embraced all men, the Irish no exception. But the limitations of antislavery ideology, especially the generally prevalent unwillingness to recognize the difficulties inherent in class and cultural distinctions among America's peoples, provided an insuperable barrier to honest and fruitful communication.

The failure of the Garrisonians was an honorable one, for their myopia was rooted in generosity. It was an essential egalitarianism—one strand in America's revolutionary heritage—that identified the causes of Irish and black freedom, an identification not popular with the Irish. The inability of the Irish to accept the equation required an attack upon their position which was temporarily damaging to Irish nationalist organization in the United States. More significant for intellectual history, however, are the limiting individualist and egalitarian assumptions in the ideology of the Garrisonians.

DANIEL O'CONNELL WAS A HERO of the antislavery movement. No European political figure of the nineteenth century was portrayed in such heroic dimensions in America. The great leader of the movement for Catholic Emancipation—the removal of those religious qualifications that had disfranchised most Irish—was seen as the exemplary international humanitarian of the age, a man whose literal belief in the brotherhood of man forbade him to take a compliant stand on American slavery for the sake of financial support for his domestic political goals from the overseas Irish. The very first issue of William Lloyd Garrison's *Liberator* quoted O'Connell on the hypocrisy of an American republicanism that supported slaveholding: "I tell the American that he is a hypocrite," O'Connell said. "*Look at the stain on your star-spangled banner. . . . I turn from the Declaration of American Independence, and I tell him that he has declared to God and man a lie.*" Garrison quoted this statement approvingly—as he would for a

generation hundreds of other speeches of the Irish Liberator and his son John. "Where is the O'Connell of this republic?" he pleaded.³

O'Connell's antislavery reputation antedated that of Garrison himself. O'Connell had been an active member of the British and Foreign Anti-Slavery Society in the early nineteenth century, and he was a key figure in the parliamentary debates in the 1830s on the abolition of slavery in the British West Indies. Though he disapproved of compensating masters for the loss of their property, he looked upon West Indian emancipation as one of the glorious acts of nineteenth-century civilization, a symbol of the inevitable progress of man. Garrisonians agreed with him, and for over two decades they ignored the Fourth of July to commemorate the final act of Caribbean emancipation of August 1, 1840. Here, "in the face of the world," as Wendell Phillips wrote, was a demonstration of "the safety and expediency of immediate emancipation."⁴ Garrisonians conceived themselves to be members of an international community of humanitarian reformers—"co-labourers as citizens of the world,"⁵ as they often said—and O'Connell was a leading figure in this world brotherhood of the just. They took pride in his support, his audacity, his renowned power to rebuke the wayward, and his widely proclaimed oratorical genius. "However Erin may be oppressed," an abolitionist wrote, "her sons carry fearless hearts and free tongues."⁶

Garrison met O'Connell in London in 1833 and enlisted him in the first major antislavery battle, the war on the American Colonization Society. O'Connell was a powerful ally. He told a mass anticolonization meeting at Exeter Hall that he always wished to visit America but would never do so while it was a slaveholding country. He asked each American who came to see him if he was a "thief" or an "honest man" and refused to shake the hand of anyone who in some way found reasons to justify bondage. It became a part of antislavery folklore for a generation that he asked the light-skinned black Garrisonian, Robert Purvis, if he was a slaveholder or not.⁷ O'Connell refused to grant an audience to the proslavery editor of the New York *Herald*, James Gordon Bennett, and was pilloried for it in that newspaper ever after.⁸ "I should be sorry to be contaminated by the touch of a man from those States where slavery is continued," O'Connell told an anticolonization gathering in 1833. "If a man should propose to me a discussion of the propriety of picking pockets, I would turn him out of my study, for fear he should carry his theory into practice." For the Irish agitator, slaveholding was an insolent interference with all notions of humanitarianism and di-

³ *Liberator*, Jan. 1, 1831, italics in original.

⁴ Wendell Phillips to George Thompson, July 1839, William Lloyd Garrison Papers, Boston Public Library.

⁵ Maria Weston Chapman to Elizabeth Pease, Aug. 20, 1839, *ibid.*

⁶ *National Anti-Slavery Standard*, July 23, 1840.

⁷ *Ibid.*, June 2, 1842.

⁸ *Ibid.*, Sept. 25, 1845.

vinity, a system geared "to fetter with law the choice which the conscience makes its mode of adoring the eternal and adorable God."⁹ He found it as unjust for the American Colonization Society to encourage free blacks to return to Africa as it was for English parishes to transport their poor. He termed Elliott Cresson, the Quaker agent of the American Colonization Society in England, a "humbug" and his society "the most ludicrous . . . that ever yet was dreamed up." This pleased Garrison. "How do you like O'Connell's speech?" he asked a relative. "Is it not just the thing?"¹⁰ Many free blacks thought it was. Blacks in New York City held a special meeting in the Abyssinian Baptist Church to honor O'Connell, whom they called the "uncompromising advocate of universal emancipation, the friend of oppressed Africans and their descendants, and of the unadulterated rights of man." On other occasions blacks in the major Northern cities repeated their acclaim in public meetings.¹¹

O'Connell was also outspoken in the 1830s, and again in the 1840s, on the expansion of slavery into Texas and the possibility of that state being recognized as an independent nation. Here British foreign policy and issues of conscience fused. O'Connell called the expansion of slavery into Texas a scheme of land piracy, a way to create three or four more slave states. He introduced a resolution in the House of Commons advising the government to withhold recognition of Texas unless slavery were abolished there.¹² He suggested the formation of an all-black colony on the Mexican border that might act as a buffer against further American expansion. When he was denounced as a protector of Roman Catholic interests in Mexico, he challenged colonialism and its *raison d'être*. "There are your Anglo-Saxon race! your British blood! your civilizers of the world . . . the vilest and most lawless of races. There is a gang for you! . . . the civilizers, forsooth, of the world!" If the Seminoles might carry on a successful war against the United States in Florida, he argued, the Mexicans, Indians, and blacks had as likely a chance on the Texas borders. O'Connell, a pacifist at home, became a warrior on America's frontier.

This is the time to form a colony of men of color to restrain the spread of slavery, to prevent the importation . . . of millions of human beings, to prevent the possibility of setting up another slave-holding state; and to discountenance in every part of the civilized world . . . the barbarous assertion of "property in man."

Edward M. Davis, the brilliant and energetic Philadelphia Garrisonian and son-in-law of Lucretia Mott, wrote that no one who read that speech could refuse to engage in social action.¹³

⁹ *Liberator*, Jan. 26, 1833, Jan. 4, 1839; *National Anti-Slavery Standard*, Aug. 27, 1836.

¹⁰ O'Connell's speech against the American Colonization Society, July 13, 1833, in *Daniel O'Connell upon American Slavery* (New York, 1860), 43-47; Garrison to George W. Benson, Nov. 25, 1833; Garrison to Henry C. Wright, Apr. 11, 1836, Garrison Papers.

¹¹ *Liberator*, Jan. 26, 1833, Aug. 8, 1845.

¹² *Ibid.*, Oct. 25, 1839.

¹³ *Ibid.*, Dec. 6, 1839; Edward M. Davis to Pease, Dec. 1839, Garrison Papers.

And what was true for Texas was valid throughout the colonized world for the Irish Liberator. Misery, crime, and devastation—the obliteration of aboriginal inhabitants by “Civilized Man”—were inevitable concomitants of Western colonialism, O’Connell told the members of the Aborigines Protection Society. He considered these among the greatest crimes of mankind and cited examples from settlements in the East Indies, Van Diemen’s Land (Tasmania), and New Zealand. Colonialism and oppression went hand in hand, O’Connell argued, and he insisted that “no other human event led to evils so multitudinous.”¹⁴

These were themes that Garrisonians understood. Although their ideology differed from O’Connell’s in some ways, the two intersected at most vital points. O’Connell conceived himself as a universal reformer; he thought no contemporary social problem, from capital punishment to temperance reform, beyond his ken. He was adamant in his insistence on moral suasion as the only useful tactic for impoverished Irishmen and enslaved blacks.¹⁵ At critical times in the 1820s and 1840s it seemed that only his influence with the Irish masses prevented massive outbreaks of violence—first on the issue of Catholic Emancipation and later on the long campaign to repeal the legislative union of Ireland and England. O’Connell, like so many Garrisonians, was an essentially middle-class reformer with a middle-class ideology. He had faith in the ultimate political advantages of constant and unyielding agitation.¹⁶ Wendell Phillips made that tactic the central focus of a famous lecture on O’Connell, which he delivered repeatedly to ecstatic Irish audiences and others through the 1870s. To Phillips, O’Connell represented better than any other man of the century “the modern element in constitutional government: agitation.”¹⁷ Phillips’s friends considered him the American version of the “Agitator,” the reformer whose wit, eloquence, and devotion to humanity were equal to those of his Irish contemporary. Phillips often compared the conditions of Southern blacks and Irish peasants in attempts to win sympathy from Irish audiences.¹⁸

Most Garrisonians admired the way the Irish Liberator maintained an incessant war on slavery and was in touch with American political issues while pursuing his fight for Irish national demands. The Garrisonians thought of themselves as Irish nationalists, too. It was not surprising, then, that American abolitionists at the World Anti-Slavery Convention, held in London in 1840, beat a constant path to O’Connell’s door. For them he was the universal reformer par excellence.

¹⁴ *Liberator*, Aug. 7, 1840.

¹⁵ O’Connell’s nonresistance, however, made a great deal of political sense for a leader of potentially volatile and violent peasant masses, while Garrison’s reflected his Christian anarchism.

¹⁶ Angus MacIntyre, *The Liberator: Daniel O’Connell and the Irish Party, 1830–1847* (New York, 1965), 3, 12–15, 46, 295–98, *passim*.

¹⁷ *New York Times*, Dec. 10, 1872.

¹⁸ *National Anti-Slavery Standard*, Jan. 22, 1870.

THE PRINCIPAL ATTEMPT to exploit O'Connell's prestige in recruiting Irish-American support for the abolitionist cause came in the early 1840s and emerged from the intoxicating atmosphere created by the Anti-Slavery Convention. The personal contacts established by Garrisonians in England and Ireland, especially those with O'Connell himself, contributed to a strong and in some ways misleading sense of international solidarity. O'Connell in person justified his reputation, and nothing in the Garrisonians' experience abroad challenged their identification of the Irish and the black freedom struggles.

It was an identification already well established in abolitionist thought and apparent in the abolitionists' plans for their journey to Britain. George Bradburn's feelings are a perfect case in point. A slightly deaf but witty and tough-minded abolitionist with a cutting tongue, Bradburn had served in the Massachusetts legislature and was as outspoken there on issues of Irish oppression in America as he was on the denial of civil rights to blacks. The Charitable Irish Association in his home town, Nantucket, invited him to speak on its tenth anniversary, and he was also commended in the Roman Catholic Boston *Pilot* for his denunciations of American nativism. Bradburn compared anti-Catholic prejudice to racism and said both arose from common sources. He wanted the Massachusetts legislature to compensate Catholics for the destruction of their property by mobs in the 1830s. He praised the Catholic settlers of colonial Maryland who had welcomed all comers while some of his Puritan ancestors were burning the churches of Baptists and hanging Quakers or cutting off their ears. Irish immigrants possessed as many admirable qualities as others, Bradburn concluded, and the prejudice against them, he insisted, would diminish in time, as they were permitted wider opportunities for acculturation.¹⁹

On the eve of his departure for London Bradburn gathered letters of introduction, some from the *Pilot* staff, to "intelligent Catholics abroad," saying he would acquire reliable information from them rather than become dependent upon the impressions of their enemies. Like so many Garrisonians, he wrote with romantic sentimentality about the especial qualities of the Irish "heart." It was an idea often expressed by Lydia Maria Child, and it became a cliché of the antislavery movement. Boasting of his Irish grandfather to his Roman Catholic constituents, Bradburn regretted having only "half such a heart." Most of all, he told his friends, he wanted to meet O'Connell. He traveled to the London convention with Garrison, Charles Lenox Remond, Nathaniel P. Rogers, Lucretia Mott, Wendell Phillips, and others. On his arrival Bradburn tracked O'Connell down and made sure of a long interview with him.²⁰

O'Connell's rousing addresses before the London convention and at other

¹⁹ *Liberator*, Apr. 30, 1841.

²⁰ George Bradburn to Francis Jackson, May 5, 1840; Bradburn to Richard D. Webb, Mar. 16, 1841, Garrison Papers.

meetings confirmed the Garrisonians' admiration for him. His most important talk dealt with the peaceful achievement of West Indian emancipation. "Did not the planters assert, the moment they [the slaves] were emancipated, violence, turbulence, slaughter, and massacre would be the consequence?" O'Connell inquired. "What do they say now? . . . Has anything approaching to spoliation of property, or to the murder of a single human being, been committed? . . . I stand here the triumphant advocate of the negro race." He went on to ask America to follow England's example, to denounce the illegal slave trade and the expansion of bondage to the American Southwest. He wanted the Texans isolated from the world community. To Garrison, O'Connell became "that sturdy champion of Irish liberty, and most wonderful among the statesmen and orators of the age."²¹

The Garrisonians sat on the sidelines of the London meeting because it failed to grant equal rights to women, but they used their time judiciously to lecture throughout England, Scotland, Wales, and Ireland. Garrison chastised the English abolitionists for supporting freedom abroad while being "indifferent to the sufferings of Ireland."²² He and the others lectured widely on universal reform, antislavery, and temperance. The most prominent figure in 1840, however, and in the few years thereafter was the black Garrisonian from Salem, Charles Lenox Remond.

Remond was a descendant of French West Indian immigrants and men who fought in the American Revolution. He was the most important spokesman for free blacks in the Garrisonian movement, and the early 1840s were the heyday of his popular leadership. An angry and embittered man until the day of his death, he delivered an untold number of addresses on the harmful impact of race prejudice. There was cause enough, he often said, in the daily treatment he received to gnarl the most gentle of spirits. He believed, along with Garrison, in a totally color-blind nation, one in which race had no influence at all. Some years later Remond opposed the appointment of a black as ambassador to Haiti, believing a white would serve with equal capacity. A universal reformer and an advocate of the brotherhood of all men, Remond wanted recognition simply as a human being, not as a black man. He dressed fastidiously, wrote letters in the most elegant script, spoke in the most florid tones, and cared much about propriety and style. His reputation and leadership were eclipsed in 1845 by Frederick Douglass, the brilliant and pragmatic fugitive slave who was less concerned with formalities and more interested in tactics of social revolution and reform and with watching for his own main chance. Before long both men came to distrust, perhaps to hate, each other. But the field was still clear for Remond in the early 1840s, and he was showered with praise throughout his European tour. "Let all geographical distinctions between nations cease," he pro-

²¹ *Liberator*, July 31, 1840; Garrison to Helen Garrison, July 3, 23, 1840, Garrison Papers.

²² *National Anti-Slavery Standard*, Oct. 8, 1840.

claimed, "so far as they serve to divide the human family into castes, and let love abound universally."²³ It was a heady moment.

As one of the early blacks to tour Great Britain, Remond received an especially warm welcome. Graciously received in circles that rejected the white Garrisonians as radicals and fanatics, he found color an asset abroad, not a hindrance, and so would a generation of slave fugitives who followed his path. Garrison made special mention of the fact that Remond always rejected invitations that excluded his colleagues. "I cannot refrain from expressing my admiration for our friend," he told a Boston audience on his return.

Though a warmer welcome than ordinarily awaits the white man was extended to him, as a man of color, he nobly refused to enter, where any of the advocates of human rights were thrust out. And, in thus deciding, he did more for our cause than he could possibly have done by neglecting to bear so emphatic a testimony.²⁴

When the other abolitionists returned home, Remond stayed behind. Much of his time was spent in Ireland appealing for antislavery support. He helped weld between the Garrisonians and Irish antislavery leaders, such as Richard D. Webb, Richard Allen, and James Haughton, an alliance that lasted until their deaths in the 1870s.²⁵

Remond also joined the circle of O'Connell's admirers. He attended O'Connell's lectures on India and listened to speeches of the British abolitionist and friend of Garrison, George Thompson. The combined energies of such individuals, he wrote, strengthened his own resolve that "God, truth, and the oppressed, will ere long conquer, prevail, and live." Remond dined with O'Connell and recorded O'Connell's testimony that color prejudice was wicked and absurd. "Let those base men who calumniate the disinterested O'Connell" know that he was a man destined to help undo slavery across the Atlantic, Remond stated. American journalists who denounced such a man as an agitator were a disgrace, he said. Never in all his years as an abolitionist had he heard such scorching rebukes of slavery. He had thought himself a fairly sturdy antislavery man until he measured his powers against those of O'Connell. "No nation or people possess a superior to Daniel O'Connell," he told a black colleague. In a series of epistles from Europe Remond repeated variations on this theme. He was enchanted with the *Irish Liberator*.²⁶

And Remond enchanted the Irish in 1841. His many antislavery speeches in Dublin, Waterford, Wexford, Cork, Belfast, Limerick, and elsewhere re-

²³ Charles Lenox Remond, quoted in Garrison to Helen Garrison, June 15, 1840, Garrison Papers.

²⁴ *Liberator*, Aug. 28, 1840.

²⁵ Garrison to Oliver Johnson, Apr. 9, 1873, Garrison Papers.

²⁶ *Liberator*, Sept. 23, 1840; Remond to Charles B. Ray, quoted in *ibid.*, Oct. 16, 23, 1840; *National Anti-Slavery Standard*, Nov. 5, 1840.

ceived wide coverage in the Irish press. Wherever he went he repeated the traditional charges of American despotism and spoke of his desire to win Irish sympathy for the abolitionist cause. He played on one of the central themes in the antislavery critique: while America seemed an asylum for the poverty-stricken of Ireland, she was, at the same time, a hellish nation for slaves and free blacks. Remond hoped to generate enough antislavery enthusiasm in Ireland to reduce the strength of Irish proslavery feeling in America. He described the workings of the slave system and the oppression of free blacks in the greatest detail. "The nominally free . . . still suffer all the pains incident to a degraded race," he told a Dublin audience.

They are deprived of political rights, or are deterred from exercising them; nearly every respectable trade and profession is shut out from them by the custom of the country; they are not permitted to occupy the inside of stage conveyances, or the cabins of steam vessels, though they offer to pay as much, or more, than their *pale-faced* countrymen. In short, every indignity that brutal prejudice and fancied superiority can inflict, is their bitter portion.²⁷

Remond and his occasional traveling companion, the brilliant and volatile John A. Collins, devised a scheme to translate Irish sympathy into social action. They helped compose "An Address of the People of Ireland to Their Countrymen and Countrywomen in America," and members of the Hibernian Anti-Slavery Society and other interested volunteers hawked it from door to door until it had 60,000 signatures; 70,000 had signed by the final count in 1842. The names of Daniel O'Connell and Father Theobald Mathew, the famous Irish priest and temperance lecturer, headed the list. The gathering of the signatures in Dublin was described in the *Liberator* in an essay, "The Irish Spirit": "It is really delightful to see the energy of some poor people in obtaining signatures," the Dublin correspondent wrote.

Two working men brought me in a sheet filled, mostly by silk weavers, and three shillings and one halfpenny donations to the cause, given by the signers in halfpence, pence, etc. A young lad, about thirteen, had been most indefatigable in collecting signatures. I heard the other day, he was going from house to house in the more genteel neighborhoods, rapping at hall doors . . . the other day he came for five sheets more. He told me he was going to school the next week, and that before he left, he must do all he could to liberate the slaves. . . . Another young man brought me in four sheets, containing amongst other names, those of forty-three Roman Catholic clergymen, of whom one is a bishop, and eleven parish priests.²⁸

The Address was short and direct, and its ideas were repeated in hundreds of letters from abolitionists in Ireland to their brethren in America for the next twenty-five years; many of the letters were reprinted regularly in the antislavery press. America, the Address argued, was a land the Irish admired.

²⁷ *Dublin's Freeman's Journal*, quoted in *Liberator*, Sept. 10, 1841, italics in original.

²⁸ *Liberator*, Oct. 8, 1841.

In the age of steam transport each nation necessarily had a growing influence on the other. Slavery, however, was a blot on all that was noble and glorious about the United States; it was the most tremendous invasion of the inalienable rights of man in modern times and a sin against God. None can be neutral on the issue, the Address continued, and “we call upon you to *unite with the abolitionists*, and never to cease your efforts until perfect liberty be granted to every one of her inhabitants, the black man as well as the white man.” The American Irish were entreated to exercise whatever political power and influence they had for the cause of humanity and to proclaim liberty for men of every color by every peaceful means. “Irishmen and Irishwomen! treat the colored people as your equals, as brethren. By all your memories of Ireland, continue to love liberty—hate slavery—*cling by the abolitionists*, and in America you will do honor to the name of Ireland.”²⁹

When Remond left England for America in December 1841 he took the Address with him. “Let me say,” Remond announced on departure,

that never were my hopes higher, my expectations stronger, or my zeal more ardent, than at present. Since my travels in Ireland I am more than ever convinced of the influence which the United Kingdom is capable of exerting, and will increasingly exert . . . for the overthrow of the iniquitous system of American Slavery.³⁰

For the next few years the Irish-American communities and the abolitionists felt the reverberations of the Address. The results were much less triumphant than Remond had hoped.

IN 1841 THE GARRISONIAN INNER CIRCLE—men and women like Wendell Phillips, John A. Collins, Francis Jackson, Charles Hovey, Anne Warren Weston, and Garrison himself—prepared diligently to exploit the Address as a tool of propaganda, a way to reach out to an ethnic group traditionally hostile to antislavery radicalism. A committee was formed to arrange a meeting with the Boston Irish. Collins interviewed the editor of the *Boston Pilot*, who reluctantly agreed to advertise the gathering, and money was raised to publish the Address in many other Irish and Roman Catholic newspapers. “I am confident that this address will do much good in this country,” Collins wrote.³¹

The Address itself, made mammoth by its signatures, was rolled into the annual meeting of the Massachusetts Anti-Slavery Society, and copies of it were lithographed for general distribution. George Bradburn suggested that a prominent place be given to the first signers, Daniel O’Connell and

²⁹ “An Address of the People of Ireland to Their Countrymen and Countrywomen in America,” quoted in *Liberator*, Sept. 10, 1841, also in *Daniel O’Connell upon American Slavery*, 38–40, italics in original.

³⁰ *Liberator*, Dec. 18, 1841.

³¹ John A. Collins to Webb, Jan. 1, 1842, Garrison Papers.

Father Theobald Mathew: "The mere sight of those names . . . would perhaps more powerfully affect the Irish among us, than all the lectures we could deliver to them," Bradburn told Francis Jackson, a prosperous businessman and one of the most conscientious Garrisonians. "There are five thousand of them in this small city [Lowell]. Might not one be almost sure of winning them over to the cause of humanity, could one go before them with that big address on his shoulders?"³²

The grand gathering was held in Faneuil Hall, the largest meeting place in Boston, with a seating capacity of five thousand. The hall was packed. It was called a "Monster Meeting," consciously modeled on the gatherings of that name which O'Connell held. Garrison, who chaired the session with his customary calm and apostolic bearing, read the Address aloud. The greatest applause from the Irish "peasants"—their dress distinguished them as clearly as that of blacks, Anne Warren Weston wrote with an unconscious touch of elitism³³—came at the names of O'Connell and Mathew. Frederick Douglass was among the many speakers, and he delivered his well-known address on the horny-handed slave talking with his gentlemanly master. When Garrison asked if anyone there would return a fugitive slave, the crowd shouted "No!" Bradburn offered a resolution in support of Ireland against English oppression. He appealed to the Irish as members of the working class, always at war with a system of slavery that attacked the interests of laboring men everywhere—a theme much repeated in appeals to the immigrants. The full implications of such a class appeal were apparently not perceived by either speakers or audience. The Garrisonian condemnation of the labor system of slavery was more often moral than economic, and current Irish agitation was not focused on any labor system as such but on the repeal of the Act of Union that had abolished Ireland's separate Parliament in 1801. Freedom was conceived in personal and national rather than in class terms. This is apparent in Bradburn's speech and in those that followed along similar lines by Remond, Phillips, James C. Fuller, Lunsford Lane, Abby Kelley, and Edmund Quincy. According to Quincy, America's Revolutionary forebears wanted to create a nation sympathetic to the liberties of all, and the Irish shared the same quest for independence in their repeal movement. Garrison re-emphasized the point, arguing that antislavery was simply America's version of "repeal"—he carefully selected that very word—and called on Irish Protestants and Catholics to forget their old-country divisions and work for emancipation in their new homeland. Remond, never free from concern with color prejudice, continued to contrast the treatment of blacks in America with the cordial reception he had received abroad. It was a rousing session, full of huzzas and great expectations. When it broke up the Garrisonians agreed that the work had been well done.³⁴

³² Bradburn to Jackson, Jan. 15, 1842, *ibid.*

³³ Anne Warren Weston to Pease, Jan. 30, 1842, *ibid.*

³⁴ *Liberator*, Feb. 4, 1842.

Lucretia Mott reported that the Philadelphia antislavery office was overrun with Irishmen seeking copies of the Address. Garrison wrote Richard D. Webb that the meeting at Faneuil Hall had produced "a great impression on the public mind." He and Remond took the Address to a special meeting in Albany, a city with a large Irish population, hoping to repeat the process. "How marvelously Providence works!" Garrison wrote characteristically. "The Irish Address, I trust, is to be the means of breaking up a stupendous conspiracy, which I believe is going on between the leading Irish demagogues, the leading pseudo-democrats, and the southern slaveholders."³⁵

But the exultation was short-lived. It was easier to tweak John Bull's nose in public of an evening and to proclaim unity with the grand nationalist movement for Irish repeal than it was to enlist the Irish immigrant in the antislavery crusade. The majority of immigrants were too busy scraping together a simple sustenance to afford the time for concern with what they judged extraneous issues. Their precarious economic position also brought them into direct conflict with working-class blacks, and violence between the two groups erupted regularly in Northern cities in the 1840s, especially but not only in Philadelphia. The bloody outbreaks of the summers of 1842 and 1844 left Philadelphia with a reputation as the "City of Brotherly Hate" and "The Murderous City." "It appears that the assaults on the persons, property and lives of the colored population were nearly all committed by the Irish residents," Garrison wrote.

It is a strange and shocking spectacle to see those, who have been forced by oppression and want to become exiles from their native land, and to take up their abode in a professedly free country, brutally combining to crush and drive out of our borders a portion of the native population.³⁶

Added to the issue of brutal economic competition was another as potentially as explosive—the dilemma of national identification. These were years of intense nativism in America, and one of the key tenets of the nativist stance was that Roman Catholic immigrants were especially prone to foreign dictation and were literally agents of the papacy in America. To survive a barrage of criticism the Irish were driven to vigorous expressions of super-patriotism and defense of the national Constitution at the very moment when the Garrisonians were trying to pull the nation apart, arguing that the Constitution was worthless. The pressures that always exist on first-generation immigrants to proclaim love for their new homeland were especially weighty for the antebellum Irish—the first massive Roman Catholic migration to the United States. Yet the Garrisonians were asking these immigrants to join a movement popularly known for its fanaticism, one that was ever ready to denounce the American Union as an abortion and that regularly

³⁵ Lucretia Mott to Webb, Feb. 25, 1842; Garrison to Webb, Feb. 27, 1847; Garrison to Benson, Mar. 22, 1842, Garrison Papers.

³⁶ *Liberator*, Sept. 2, 1842.

attacked the traditional Irish political and clerical leadership in America for leading the masses astray and hindering social progress. The Garrisonians asked the immigrants to put their lives in triple jeopardy. The immigrants were quite unlikely allies, as the movement to wed antislavery to repeal demonstrated in the 1840s.

No ostentatious flag waving could conceal the deep interest of the American Irish in the issue of repeal. Repeal Associations were formed throughout the North and South, and money was raised—"Repeal Rent"—in America to support the cause abroad. National meetings of local associations from the North and South gathered in Philadelphia, Boston, and New York City. The son of President John Tyler was active in the movement. Repeal, in fact, was the first major Irish nationalist movement of consequence in America.

The Garrisonians consciously planned to press hard on the contradictions between Irish proslavery and support of repeal. Garrison, saying he was both an "Irish Repealer" and an "American Repealer," wanted the immigrants to follow his lead, not that of American politicians or of Irish-American clergy. "The Irish population among us," he wrote a close relative,

is nearly all "democratic." . . . The democratic party is openly and avowedly the defender and upholders of the "peculiar institution of slavery." . . . The cry in favor of Irish Repeal is now raised extensively throughout the South, and sustained by the leading democratic journals,—and why? To secure the aid of Irish voters on the side of slavery, and to bring their united strength to bear against the anti-slavery enterprise! Also, if possible, by sending over donations to Ireland, to stop O'Connell's mouth on the subject of slavery, and to prevent any more "interference," on that point from that side of the Atlantic. Hence, I observe, at the Repeal meetings in various parts of the country, resolutions and declarations which amount to sacred pledges, that these "repealers" will stand by southern institutions, at all hazards! Now, by the Address . . . we shall be able to probe this matter to the bottom. If O'Connell and our friends in Ireland remain true to us, and renew their spirited attacks upon American slavery, and cry out against this unholy and frightful league between southern slave-drivers and his countrymen in America, then it will put down at the South this pretended sympathy for Ireland, and be the means of advancing our movement still more rapidly.

To Garrison, the man who shouted repeal and supported slavery was a loathsome hypocrite, a phrase he used with alacrity.³⁷

The Irish-American reaction was predictable. At first the Catholic press denied the authenticity of the Address. Then the abolitionists were denounced as fanatics, lunatics, and infidels. Garrison was called an atheist, a traitor, and a destroyer of the social order. He should "be immediately transported to Ethiopia, there to dwell in all love and harmony with the wild negroes," the *New-England (Catholic) Reporter* declared.³⁸ He was recom-

³⁷ *Ibid.*, Apr. 8, 1842; Garrison to Benson, Mar. 22, 1842, Garrison Papers.

³⁸ *New-England (Catholic) Reporter*, quoted in *Liberator*, Nov. 11, 1842.

mended to an asylum for treatment of “maniac ultraism.”³⁹ The *Boston Pilot* feared the mingling of the issues of slavery and repeal and insisted that no pretext would induce Repealers to ally themselves with any other cause. “The high admiration they feel for the essential characteristics of the American Constitution, is too deep and controlling to allow them to engage in a question which imperils the only free government in the world.”⁴⁰ The *Boston Catholic Diary* said it revered O’Connell and Father Mathew, but “we can tell the abolitionists that we acknowledge no dictation from a foreign source.”⁴¹ A number of commentators insisted that the Irish were in worse condition than the slaves, a claim repeated for a generation. Others said such talk might lead to race warfare. All stressed their first devotion to their new land of settlement and considered as enemies those who addressed them as a special interest group in the United States.

A meeting of Irish miners in Pottsville, Pennsylvania, brought together most of the key arguments. They called the Address a fabrication, and they were not prepared to look upon Negroes as their “brethren.” If slavery existed in America, they asserted, it was entailed on this country by the English. But most of all they regarded as enemies those who addressed them from abroad upon questions of national policy. “We consider its style and manner insulting to our dignity as men—that we altogether and most emphatically declare our disapproval of it, and most solemnly disavow any intention to participate or cooperate with the authors of it,” they replied.

We do not form a distinct class of the community, but consider ourselves in every respect as CITIZENS of this great and glorious republic—that we look upon every attempt to address us, otherwise than as CITIZENS, upon the subject of the abolition of slavery, or any subject whatsoever, as base and iniquitous, no matter from what quarter it may proceed.⁴²

Such a position, of course, was the convenient half-truth, the defense mechanism of all first-generation ethnic groups. The Irish of Pottsville were indeed Irish—they gathered together as such, and they continued later on to send contributions abroad in support of repeal. The Garrisonians cleverly worked on the logical inconsistencies involved in such a dilemma and arranged to publicize regularly O’Connell’s denunciations of his followers in America. By so doing they placed the Repeal Associations in such a precarious position, making them liable to charges of questionable patriotism and foreign dictation, that one by one they collapsed. Garrison and Bradburn were wrong in predicting that a miraculous alliance between the abolitionists and Irish would occur because each supported Irish national demands. What

³⁹ *Boston Pilot*, quoted in *Liberator*, May 26, 1843.

⁴⁰ *Boston Pilot*, quoted in *Liberator*, Feb. 25, 1842.

⁴¹ *Boston Catholic Diary*, quoted in *Liberator*, Mar. 4, 1842.

⁴² *National Anti-Slavery Standard*, Mar. 24, 1842.

they accomplished instead was a considerable, generation-long disruption of the Irish nationalist movement in the United States.⁴³

THE ABOLITIONISTS' MAJOR EFFORT against the proslavery position of the Repeal Associations began in mid-1843 and lasted through 1845. Together with their attempts throughout the antebellum years to allow for Irish-American difficulties, their effort tells much about their concept of freedom and their social outlook. Freedom was a matter of national independence and individual liberty, and the latter should flow naturally from the former. Thus the principles of the Revolution required support for both abolition and repeal. They also condemned such obstacles to freedom as the anti-Catholic and antiforeign prejudices of the nativists. To admit inequalities of freedom as inherent in class membership, however, would have denied freedom as an attribute of the individual. Let us free the black, urged the abolitionists, in effect, so he may have the same opportunity as the Irishman to rise by hard work and merit. Here was an individualist, middle-class work ethic that automatically banned any class approach to relief of Irish-American economic conditions. Yet freedom was an ultimate moral end, and means, not ends, were compromised. And so the drive against proslavery Repealers was pushed.

The campaign was hardly under way when a temporary misunderstanding with O'Connell appeared to threaten it. Garrison admired O'Connell, but the Irish Liberator had serious doubts about some aspects of the ideology of his American antislavery sympathizers. While they shared similar views on repeal, antislavery, and nonviolence, much about O'Connell's faith and tactics differed radically from Garrison's ideology. O'Connell was, first of all, a masterful politician and tactician. He knew how to organize the masses for political action and also how to compromise, as he had shown in accepting a higher property qualification for the vote in order to secure Catholic Emancipation. O'Connell's eye was as much on the main chance in British politics as on the heavens and eternal justice.⁴⁴ He was also quite orthodox in his religious faith, and for a time in 1843 he openly denounced Garrison for his antisabbatarianism and anticlericalism. Having read Garrison's effusions on Christian anarchism and taking them as representative of the entire abolitionist movement in America, he determined "not to mix up his cause" with such views.⁴⁵

Gerrit Smith, the New York abolitionist, responded immediately. He told O'Connell that Garrison's private views on other issues had nothing to do with his antislavery leadership and that if Garrison's beliefs were defined

⁴³ See *ibid.*, July 20, 1843.

⁴⁴ Eric Strauss, *Irish Nationalism and British Democracy* (London, 1951), 88-106.

⁴⁵ *Liberator*, Apr. 14, 1843

in religious terms alone he came closest to being a Quaker. Whatever O'Connell heard about antislavery hostility for the Irish immigrant was untrue, Smith declared.⁴⁶

Garrison himself replied directly to O'Connell. He reminded him of their cordial meeting in 1833 and explained that he deemed it a great honor to read the Address signed by O'Connell to audiences throughout the North. His religious views, as Smith had claimed, were similar to those of the Friends, and in his own fashion he was a deeply religious Christian. Garrison completely denied that his movement was touched with nativism or was in any manner hesitant to work with the Irish immigrant as a brother. He said, "Instead of being hostile to such of your oppressed countrymen as have come hither, they [the abolitionists] are the only persons who really respect or sympathize with them; and so far from being bigoted against Catholicism, they have surprisingly emancipated themselves from sectarian bondage."⁴⁷

The crisis, thus somewhat smoothed over, was followed by a series of powerful antislavery addresses to the Irish in America by O'Connell and his son John. James Haughton, an Irish Unitarian, a friend of Garrison, and an active Repealer in Dublin, acted as intermediary between the American abolitionists and O'Connell. He and two other Irish abolitionists, Webb and Allen, regularly visited the Irish Liberator and secured from him a series of ringing abolitionist addresses to repeal organizations in the United States. When O'Connell was imprisoned for his agitation in Ireland, Henry C. Wright went to visit him in jail.⁴⁸

The turning point of the campaign in America began in June 1843. O'Connell replied to an address of the Pennsylvania Anti-Slavery Society that accused the Irish-American Repealers of proslavery inclinations. In a blistering speech he repudiated the support of those who believed in the "aristocracy of skin." He was sorry, he said, that "there were Irishmen in America who had taken the wrong side with regard to the liberties of the human race." He read the proceedings of the Pennsylvania Repeal Association—with its plea to the immigrant to support the slaveholding Union—and was deeply sorrowed by it. It tarnished the cause of Irishmen in Ireland. "I wish the Anti-Slavery Society in America should know, that this association were in no way participators in the sentiments . . . put forth" by those claiming to be repeal representatives in America. It was foolish to blame the English for establishing the system of bondage, he continued. The bondage was spread under the American flag, and that was where the responsibility rested.

No man shall dare say that such beings shall be made the property of their fellowman, and treated, not as human beings, but as the brute beast. . . . The

⁴⁶ *Ibid.*, Apr. 28, 1843.

⁴⁷ *Ibid.*, Dec. 8, 1843.

⁴⁸ *Ibid.*, Oct. 11, 1844; Webb to Garrison, Feb. 27, 1842; Allen to Garrison, Apr. 2, 1842, Garrison Papers.

man who will do so belongs not to my kind. Over the broad Atlantic I pour forth my voice, saying—Come out of such a land, you Irishmen, or if you remain, and dare countenance the system of slavery . . . we will recognize you as Irishmen no longer.

O'Connell's son John, his political heir, echoed the views of his father: "Surely no genuine son of the Emerald Isle will now be found on the side of slavery," he wrote. "He who, when he leaves the land of his birth . . . so far forgets himself . . . is a recreant to his country, and, as such, we fling even the recollection of him from us. . . . Has he so soon forgotten the evils of oppression at home?"⁴⁹

This was all the Garrisonians needed. These speeches were reprinted for massive distribution. A reading room to attract the Irish was set up in Philadelphia. O'Connell was himself again, Garrison crowed. "The whole speech is throughout worthy of the best days and the best efforts of Daniel O'Connell," he said.⁵⁰

O'Connell delivered a series of similar rebukes in October and December 1843. Proslavery money would not stop his mouth, he said, and a slaveholding Repealer was nothing but a farce.⁵¹ His most vituperative speech came in 1845 on the question of Texas annexation. "I want no American aid, if it comes across the Atlantic stained in negro blood," O'Connell proclaimed.⁵² The Garrisonians again attempted to call meetings with Repealers to convince them with oratory that antislavery was in their best self-interest, but these sessions turned into battles between quite obviously opposing forces.⁵³ Neither tirades from O'Connell nor cheers by Wendell Phillips for a papal bull in 1842 denouncing the slave trade⁵⁴ could overcome the reality of the social condition of the Irish in America. The working-class, Roman Catholic, poverty-stricken, and much-abused Irish immigrant could not afford the luxury of political radicalism.

By pushing for an alliance between antislavery and repeal the abolitionists merely alienated the Irish further from their cause. Each address by O'Connell was immediately attacked by the Roman Catholic clergy in America. "If O'Connell and his associates are misled by the rampant fanaticism of this set of men," the *Boston Pilot* editorialized, "it must be the source of deep sorrow to all true friends of Ireland, and if under the excitement attendant upon the falsely colored pictures of the abolitionists, they are betrayed into any violent attack upon the American people and their countrymen here, it cannot but have a painful influence."⁵⁵

⁴⁹ *Liberator*, June 9, 1843; *Dublin's Freeman's Journal*, quoted in *National Anti-Slavery Standard*, Apr. 15, 1841.

⁵⁰ *Liberator*, June 9, 1843.

⁵¹ *Ibid.*, Nov. 24, 1843.

⁵² *National Anti-Slavery Standard*, May 8, 1845.

⁵³ *Ibid.*, Jan. 4, 1844.

⁵⁴ *Liberator*, Nov. 24, 1843.

⁵⁵ *Boston Pilot*, quoted in *Liberator*, June 23, 1843.

The resulting demise of the repeal organizations, unintended by either O'Connell or the Garrisonians, reveals Irish vulnerability to charges of conflicting loyalties. Those in Charleston, Natchez, and New Orleans succumbed in 1843, and those in Cincinnati, Philadelphia, and New York in the next two years. The only Repeal Association to survive O'Connell's bitter speech on Texas in 1845, in which he spoke of the American Irish as polluted and disgraceful, was that in Boston, but even it lasted only a short time. The *Boston Pilot* published the speech, but its editor admitted that he would have suppressed it if he could. We are "bound to say that there is not an Irishman in America who does not scout with scorn his observation about the banner of our hearty and entire allegiance."⁵⁶

By this time it was apparent to the Garrisonians that Irish-American hostility to the antislavery movement could not be overcome. Garrison and others said later that they could not remember a single Roman Catholic priest who spoke out in their defense. "It is a most deplorable circumstance that religiously and politically, almost the entire body of the Irishmen in this country are disposed to go with the accursed South for any and every purpose and to any extent," Garrison wrote Webb in 1845. "They are a mighty obstacle, therefore, in the way of negro emancipation on our soil. Truly, they know not what they do."⁵⁷

As is well known, the ultimate rejection came when Father Mathew visited America in 1849 and after an interview with Garrison and others refused to join in a celebration of West Indian emancipation. The powerful temperance advocate had been admired almost as much as O'Connell, and his activities had been recorded regularly in the antislavery press for years. But apparently he had learned the lesson of the repeal agitation well. There was no way to befriend the Garrisonians and simultaneously influence audiences of Irish immigrants. Mathew told Garrison he had too much trouble saving men from intemperance to risk clouding the issue with antislavery remarks. His first duty was to the Church, he said, and he had nothing more to do with the abolitionists during his entire stay. He was, of course, denounced as a moral coward, and Garrison poured forth several thousand words in open attack on him.⁵⁸ O'Connell's speeches were dredged up—as they would be at critical moments through the Civil War and Reconstruction—and the *Liberator's* greatness was contrasted with Father Mathew's small-mindedness. Another antislavery saint had failed in his duty before the political and social realities of America, and the moral reformers never let him rest in peace.⁵⁹

The failure of the appeal to the Irish on principle and conscience alone and the inability of so many Garrisonians to understand clearly the social

⁵⁶ *Boston Pilot*, quoted in *Liberator*, May 2, 9, 1845.

⁵⁷ Garrison to Webb, Mar. 1, 1845, Garrison Papers.

⁵⁸ See *Liberator*, Aug., Sept. 1849.

⁵⁹ See, for example, C. M. Burleigh to Sidney Howard Gay, Aug. 16, 1849, Sidney Howard Gay Papers, Columbia University.

condition of first-generation Irish in America and the reasons for their political conservatism were capsulized in a fascinating exchange between Garrison and Michael Sheehy in 1857. Sheehy, an old follower of O'Connell and an antislavery and temperance man, arrived in America in 1853. He wrote a lecture entitled "O'Connell, the Emancipator of the British West Indies Slaves—The Eloquent Advocate of Freedom for All of Every Color, Caste, Clime," which he delivered at his own expense to Irish audiences in Taunton, Fall River, New Bedford, Boston, and elsewhere. And so he alienated potential friends. When Garrison met him, Sheehy was living in a basement on Williams Street in New York City and suffering the curses of Job: his wife had a tumor, his own right hand was crippled, and he was completely without friends.

Garrison raised money for him, but rather than give it to him directly, he supervised the fund as a benefactor. He indulged in a detailed description of the reasons behind his interest in Sheehy's case. "I feel the more interested in Mr. Sheehy because he has maintained his anti-slavery integrity, to his own cost, where almost all his countrymen have proved recreant," Garrison wrote. He found a new home for Sheehy and filled it with furniture, but he retained the legal ownership of all the possessions. Then he lectured the troubled Irishman on the sanctity of the bourgeois ethic. He told him to get a job: "Indeed it is indispensable that you should find something to do, for individual charity is soon exhausted, and toil is the price that the poor pay for bread." Garrison also claimed that he took an interest in Sheehy because he was a middle-class man unfamiliar with such degradation and because he was an Irishman, "which in this country, is next to being of African extraction." Garrison concluded in the mood of an insufferable moralist: "May you ever retain a character for uprightness and be willing to die of starvation rather than sell your manhood to obtain food or favor!"⁶⁰ The choice of ideology or food was one the overwhelming majority of Irish immigrants were unwilling to face, if they had anything to say about the matter.

O'Connell died in 1847, and repeal in America, on the wane since 1845, expired with him. Its death was due partly to the abolitionist onslaught and partly to changes in Ireland where the Great Hunger of the 1840s was producing new tactics and leadership. O'Connell's passing deprived the anti-slavery movement of the key figure so admired by the immigrants. The abolitionists no longer had direct access to political leaders in Ireland who sympathized with their point of view. From then until the end of the Civil War the Garrisonians kept in contact with only a small group of Irish abolitionists, largely Quakers and Unitarians. These men remained true to the cause of antislavery in America; some visited America and corresponded regularly with the abolitionists through the 1870s, but they had no political influence at home or abroad. In O'Connell the antislavery arsenal lost a chief

⁶⁰ Garrison to Michael Sheehy, June 18, 1857, Garrison Papers.

weapon. The more radical Irish revolutionaries who rose to power in the late 1840s felt no concern at all with American issues and were hostile to the middle-class leadership and ideology that O'Connell had stood for. Young Ireland, the Irish Forty-eighters, were more often than not consciously hostile to the Garrisonian movement.

"JESUITICAL," LIKE "RABBINICAL," was a term of opprobrium used regularly in antislavery newspapers and correspondence. Synonymous with clerical leadership and dictation of all kinds, the term was applied as often to Protestants as to Roman Catholics and carried no particularly anti-Catholic connotation, though a modern reader of antislavery writings will find other suggestions of anti-Catholicism. Garrisonians were especially opposed to all religious sanctions on free investigation, and in the course of forty years many articles in their newspapers refer with hostility to "priestly" or "papal" direction. A diligent reader of the *Liberator* and the *National Anti-Slavery Standard* will also encounter numerous puns based on the Irish immigrant as drunkard and fighter. Certainly a deep strain of anti-Catholicism ran through the brilliant mind of a man like Theodore Parker.

But such facts obscure the central theme—the one of which Garrison wrote O'Connell—that the most active abolitionists overcame within their human limits the anti-Catholic prejudices inherent in any predominantly Protestant evangelical social movement in the nineteenth century. Despite numerous physical provocations, despite the Irish-Americans' rejection of abolitionism, despite the prominent role of Irish marines in guarding and returning Anthony Burns to slavery in 1854—a moment of sacrilege to abolitionists—despite the Draft Riots themselves, the Garrisonians rarely slipped into an nativist stance. When they spoke of the brotherhood of all men they included the Roman Catholic Irish. In fact no more forceful attacks on nativism and Know-Nothingism came out of antebellum America than those of the Garrisonians. Lydia Maria Child said she had never heard an Irishman called "Paddy" without experiencing a feeling of disgust. Youngsters were more likely to learn about the moral sentiments of the nation from such talk, she said, than from formal instruction in school.⁶¹

The *Liberator* printed an unusually sympathetic series of articles in 1835 entitled "The Irish Population." The Irish immigrant was seen as seeking refuge from poverty and destitution, and all he had to work with was his hands. "He is ready for labor; he shrinks not from the lowest, the most painful, the most menial, which is also . . . the worst paid," the essay argued. He hauled freight, worked on the docks in the winter, and was generally industrious and frugal. And despite this heavy economic burden the Irishman was the special target of prejudice.

⁶¹ *National Anti-Slavery Standard*, June 3, 1841.

Prejudice arms against him the native laborer, because the Irishman is willing to work cheaper; prejudice excites the sectarian against him, because he continues to be attached to a church, in which he is brought up, and from whose ministers he has received what little kindness he ever experienced from the classes above him.

When mobs attacked the Irish and burned their convents, the essay concluded, each abolitionist should ask himself if he in any way contributed to such a religious hatred.⁶²

Another article dealt with the image of the Irish as a naturally belligerent people. Much of this impression was attributed to information supplied by their English oppressors, and it was explained that as the Irish worker often experienced unfair and brutal treatment from his employer, he naturally felt the instinct to retaliate. When a people suppressed so long should finally be treated with fairness under the law, they would prove as valuable an addition to America's population as other immigrants.⁶³

The final article of the series attacked a public message by the governor of Massachusetts complaining that paupers were being forced to emigrate to the United States. Let them come, the author stated, and settle in the cities as the wealthier and more enterprising natives go west. Since the governor had complained of immigrant idleness, a nameless abolitionist asked whether he had spoken with any railroad builders in recent years. Was America to brand the poor and exclude them and welcome only the upper classes? "After eighteen hundred years of Christianity, how much better is its spirit understood now?"⁶⁴

To the Garrisonians, Know-Nothingism was a political charade whose mask hid proslavery powers. It channeled the energies of potential anti-slavery advocates into a false concern; it made men heated over immigration and threats of foreign invasion when they should recognize in slavery the vital issue. Edmund Quincy wrote a markedly insightful editorial against Know-Nothingism. "The Know-Nothing Movement is one of those temporary excitations which grow up in a night, and wither when the sun is high in the heavens," he declared.

It has no principle of enduring vitality. . . . Either success or defeat will be fatal to its life. No party can prosper on so very small a capital as hatred to a Sect so much in the minority as are the Catholics. . . . The occasion is not sufficient. The evils are rather imaginary than real.

All that Know-Nothingism could accomplish, Quincy continued, was to divert the attention of the masses from "the real question of the age." Hatred of popery was endemic to American life, easily appealed to, but as easily cast aside. The best Quincy could hope for was that nativists would

⁶² *Liberator*, Jan. 3, 1835.

⁶³ *Ibid.*, Jan. 13, 1835.

⁶⁴ *Ibid.*, Feb. 7, 1835.

damage the traditional parties beyond recognition and call forth new ones that would deal with slavery. Perhaps, in some twisted way, Quincy concluded, a new unity might emerge from the present chaos.⁶⁵

When a Know-Nothing legislature and governor were swept into office in Massachusetts in 1854, the *Liberator* called the party a ridiculous satire. It was a party that burrowed in the earth like a mole; an incredible alliance of the most unlikely forces; a movement that tried to silence the slavery debate and was basically proslavery in spirit—putty in the hands of those who wished to shape it for political advantage.⁶⁶ From 1854 through the demise of the Know-Nothing party in Massachusetts in 1859 the *Liberator* kept up its constant assault. The main reason was ideological: Garrison said Know-Nothingism sought to proscribe the sacred right of all men to worship God in their own ways; it was an invasion of an inalienable right of man and unjust and unconstitutional as well. He defended the right of Roman Catholics to use their version of the Bible and prayer books in the common schools. Garrison personally would have no Scripture reading in the schools at all, but since Protestants insisted on it, the Roman Catholics had equal rights to state their own case. Garrison wrote to Karl Heinzen, the German-American antislavery editor in Boston, that he was opposed to the seven-year naturalization law being considered in the legislature of 1859. Men should have access to the franchise as soon as they were capable of understanding issues in public debate, he said. Gerrit Smith reiterated the argument in fulsome detail.⁶⁷

Tactical reasons also dictated the Garrisonian assault on the Know-Nothings. Know-Nothingism was for some antislavery men a bridge to the Republican party. Garrison recognized familiar faces when he visited the Massachusetts legislature in 1855—and for the first time received a cordial welcome there. The Know-Nothing legislature actually voted the removal of Edward G. Loring for his role in returning Anthony Burns to slavery. It also permitted, after a decade of agitation, the integration of the Boston public schools. There were good men in the party, Garrison said, common people swept into office at a moment of national turmoil. The majority in the North were not consciously evil as much as bewildered. The Garrisonians recognized a threat to their antislavery goals in Know-Nothingism, a party that wooed and misled many individuals with abolitionist inclinations.⁶⁸ Men like Garrison and Gerrit Smith never stopped castigating the party and, for that reason, too, welcomed its demise.

IN THE THREE DECADES before the Civil War, events were forcing new perspectives on America's national heritage. If O'Connell's record on West

⁶⁵ *Ibid.*, Nov. 10, 1854.

⁶⁶ *Ibid.*, Nov. 17, Dec. 1, 1854.

⁶⁷ *Ibid.*, Dec. 22, 1854; Dec. 17, 1858; Jan. 7, Mar. 25, Apr. 8, 1859; Mar. 16, 1855.

⁶⁸ *Ibid.*, May 4, 1855.

Indian slavery—and his strategic importance vis-à-vis the new Irish population in America—made him the reformer par excellence for the Garrisonians, others such as Mazzini, Kossuth, and Garibaldi were also hailed as liberators and were similarly perceived through the distorting glass of American ideology. This was a romantic nationalist tradition that tended, though imperfectly, to fuse concepts of national independence and personal liberty and that was best symbolized by the Declaration of Independence. The American Revolution was conceived to be a model for future revolutions in Europe. It was assumed that when a people achieved independence they would obviously set up a governmental system guaranteeing personal freedom and civil rights to all. Here lay the basis for the Garrisonian identification of Irish repeal, a step toward national independence, and abolition—personal freedom for blacks as a logical result of America's national independence.

Events, however, were violating the assumptions of the romantic nationalist tradition. Neither Irish-Americans nor nativist Know-Nothings saw any necessity to extend personal liberty to all; blacks or new arrivals—and by logical extension any other group—could be excluded from the nation and thereby denied freedom. The Garrisonians, for whom personal freedom was a universal moral end, found this illogical, but others, if judged by their acts, apparently considered freedom pragmatically as an advantageous condition to be achieved for themselves and their own group. In the European context the revolutions of 1848 more than suggested that national independence and personal liberty were not one. Whether many Americans were aware that Kossuth appealed to the American Revolutionary example to justify a Magyar regime at the expense of Slavic minorities is problematical. But the stance of the Irish Forty-eighters made the point forcefully enough at home.

Despite the effort of the Garrisonians to understand the situation of Irish-Americans and to defend them against religious and nativist prejudices, they were unable to view the immigrants' attitude on slavery as other than a moral failure. Moreover a middle-class, individualistic conception of personal freedom hindered any adequate response to the economic conditions of the Irish, though this limitation was scarcely peculiar to abolitionists. Given the individualist attitude of the period, it is difficult to see what course toward the Irish a pragmatic, nonmoralistic social analysis could have suggested—except to refrain from any attempt at recruiting their support or contesting their position.

The basic dilemma was that national independence did not necessarily guarantee personal liberty, and it would soon force itself upon all Americans in the form of the South's claim to the right of national self-determination. The logic of the abolitionists' position and their attack on the Constitution placed the personal value before the national. The problem, however, was not how to choose but how to reconcile, how to harmonize a universalist and

egalitarian notion of personal freedom with national self-determination. The abolitionists' efforts to solve their dilemma in their relations with the Irish immigrants and the Know-Nothing nativists trace the decline of the romantic nationalist tradition and foreshadow the Civil War.

Women and War in Japan, 1937-45

THOMAS R. H. HAVENS

AMONG THE MANY FORCES that jolt modern society, warfare is perhaps the most prevalent, possibly the most calamitous, and probably the most consequential. At first glance war seemingly regiments, confines, and inhibits peoples from pursuing their everyday activities; paradoxically it also propels enormous changes in the relationships among the groups and classes that constitute a society. Ever since Versailles, writers have been especially careful to accord war its due place in the history of society, characterizing it variously as a historical abnormality, a permanent social institution, a man-made disaster, or a moral outrage.¹

Since the Second World War has been the most general and the most destructive conflict to date, its social effects have been carefully examined. Gordon Wright and Richard Polenberg have written recent books that summarize conditions on the home front in Europe and the United States during World War II. Both works thoughtfully discuss war as an agent of historical change for the populations involved in the fighting. And for an even broader view of the matter, there are the abundant social consequences of World War II in the Pacific theater to consider as well.² In this

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¹ During the 1920s and 1930s the Carnegie Endowment for International Peace sponsored *The Economic and Social History of the World War*, under the general editorship of James T. Shotwell; it ran to approximately 150 volumes and was published principally by Yale University Press. In 1940 the multidisciplinary approach to the concept of total war was developed in Willard Waller, ed., *War in the Twentieth Century* (New York, 1940). Rejecting the notion of war as an aberration was Jesse D. Clarkson and Thomas C. Cochran, eds., *War as a Social Institution: The Historian's Perspective* (New York, 1941), a pathbreaking work sponsored by the American Historical Association. In *Man and Society in Calamity* (New York, 1942), Pitirim A. Sorokin foreshadowed theories of war as disaster developed in such volumes as Fred C. Iklé, *The Social Impact of Bomb Destruction* (Norman, 1958), and Allen H. Barton, *Communities in Disaster* (Garden City, 1969). The intellectual turmoil produced by the Vietnam War yielded fresh assessments of the effects of war on society in Morton Fried, Marvin Harris, and Robert Murphy, eds., *War: The Anthropology of Armed Conflict and Aggression* (Garden City, 1968).

² Gordon Wright, *The Ordeal of Total War, 1939-1945* (New York, 1968); Richard Polenberg, *War and Society: The United States, 1941-1945* (Philadelphia, 1972); for a fascinating account of China at war during 1940-41, see Graham Peck, *Two Kinds of Time* (Boston, 1950).

article I will attempt to answer two questions about wartime Japanese society. First, what were the experiences of women, Japan's largest social group, during 1937-45? Second, and necessarily more tentatively, what were the implications of wartime life for Japanese society and modern society in general?

World War II clearly had a vast social and psychological impact on citizens in all the belligerent nations. Whatever the particularistic colorations of Japanese historical development, there is no inherent reason why the conditions of daily life for Japanese women should be treated separately from those of women battered by the war elsewhere. Gauging the effects of war on a society is difficult and yields a regrettably fragile and imprecise basis for multinational comparisons. Still it is worth inquiring about the reality and the import of daily life for certain wartime Japanese women as a step toward more general conclusions about the connection between war and the history of society.

Living through a war of total national mobilization is brutalizing for a people as much because of its unpredictability as for its certainty of suffering. Modern technology has made the home front, too, a battlefield, trapped by fear, deprivation, and threats to life itself. The resulting deep psychocultural changes often strain conventional canons of hierarchy and behavior, even when authoritarian wartime leaders try to regiment the public and block social change. Hence it is moot whether a total war more greatly ossifies or transforms a people in response to national crisis.

The collision of traditional expectations and *de facto* social convolutions was especially jarring in the case of Japanese women during World War II because customary female social roles conflicted with the requirements of the war effort. Japanese women suffered deprivations neither more nor less than other citizens, but their experiences were necessarily somewhat different from those of men. Between 1937 and 1945 they were buffeted by the interplay among three main forces: national mobilization, patriarchal attitudes, and the hardships imposed by a war economy. Women were the objects of both far-reaching governmental mobilization schemes and static, male-dominated outlooks expressed by Japan's military rulers and society at large. Most important, however, Japanese women were the victims of strong socioeconomic changes induced by war. The extent to which their fortunes were altered by the fighting probably depended more on the specifics of wartime living than on the state's efforts to marshal their support or the transformation of attitudes about male dominance.

FROM THE ONSET of war with China in July 1937 the Japanese government rallied civic-minded women's associations for traditionally female duties in wartime: seeing off war-bound soldiers and sailors at dockside, comforting wounded veterans and bereaved families, encouraging economic self-suf-



Fig. 1. A Japanese army soldier training neighborhood women wielding bamboo spears in preparation for defending the homeland against American invasion, spring 1945. Photograph courtesy Kikuchi Shunkichi.

ficiency and patriotic savings movements, and opposing “the penetration of dangerous ideas.”³ During the first years of the war Japanese mobilization was fully as ideological as it was economic, and accordingly all women’s organizations were obliged to support the National Spiritual Mobilization (Kokumin Seishin Sōdōin) initiated in September 1937, or face suppression.⁴ When the war crisis deepened, a single Greater Japan Women’s Association (Dai Nihon Fujinkai) was proclaimed on February 2, 1942, reflecting the energetic attempts of the prime minister, Konoe Fumimaro, to streamline governmental administration and to paper over political opposition through his Imperial Rule Assistance Association (Taisei Yokusankai), created on September 27, 1940. It is hardly necessary to accept the subsequent claim that the women’s groups merged because “voices swelled up from the masses of women seeking a rational women’s organization,”⁵ for just as Konoe’s Yokusankai reduced but did not eliminate bureaucratic rivalries, the merger of the women’s associations failed to integrate the women’s movement as a whole.

Although Japan belatedly geared its economy for full-scale war in 1942,

³ *Nihon kindaiishi jiten* (Dictionary of Modern Japanese History) (Tokyo, 1958), 351. See Inoue Kiyoshi, *Nihon joseishi* (History of Japanese Women) (Tokyo, 1967), 289; and Shimonaka Yasaburō, ed., *Yokusan kokumin undōshi* (History of the National Imperial Rule Assistance Movement) (Tokyo, 1954), 1058.

⁴ *Nihon kindaiishi jiten*, 351.

⁵ Shimonaka, *Yokusan kokumin undōshi*, 1057. See Inoue, *Nihon joseishi*, 291.

the amalgamated women's groups continued their early wartime pattern of sponsoring lectures, promoting health programs, and conducting fund drives. Clearly they were suited more to the demands of social stability than to mustering women for war service. The standard history of the Imperial Rule Assistance Association plaintively dismisses the Greater Japan Women's Association as ineffectual because it duplicated the work of other groups, was too centralized, and lacked enough female leaders⁶—a bureaucratic monster with little proclivity or aptitude for marshaling women for modern war. It is little wonder that few persons paid heed when it was quietly dissolved, amid the rubble of almost certain defeat, in June 1945.

Although the Japanese state encouraged housewives to join traditionally female organizations, the slim supply of civilian manpower, especially after mid-1943, forced the government to consider putting more women to work. This was done to a remarkable degree in America, for example, during the same period, not without disruptions but certainly to good effect.⁷ State labor policy in Japan's equally male-controlled economy revealed more sharply than in the United States the disequilibrium between customary outlooks on women's status and the radically new demands of total war. Japanese women were unquestionably put to extensive service during the last two years of the conflict. Yet considering the grave constraints imposed by deteriorating output as the fighting wore on, it is puzzling that the Japanese cabinet to the very end avoided forcibly mobilizing the labor of women. Whether patriarchal attitudes within the state or among the public at large were chiefly to blame for this reluctance is moot. Wartime Japan seemingly matched contemporary fascist states in its preference for patriarchy, yet others have claimed that "where the state is strong the family is weak and the position of women is good"⁸ and vice versa. Perhaps the clouded relationship between authoritarianism and male dominance is less a matter of politics than one of style and mores. If the Nazis suppressed women for reasons of temperament, not economics or politics,⁹ nearly everyone in wartime Japan from military oligarchs on down appeared to hold static psychocultural views about the place of women in society. Doubtless this fact to some degree inhibited the state from legally requiring the services of all women as a part of its general national labor policy.

According to the last peacetime census (taken in 1930) before Japan's military adventures in China, approximately 15 million women were gainfully employed and more than 17.6 million were not. Of those who worked, more than 60 per cent labored in agriculture, mainly as housewives who

⁶ Shimonaka, *Yokusan kokumin undōshi*, 1071–74.

⁷ William H. Chafe, *The American Woman: Her Changing Social, Economic, and Political Role, 1920–1970* (New York, 1972), 135–73.

⁸ Bertrand Russell, "Style in Ethics," *Nation*, 118 (1924): 197–99, quoted in Kate Millett, *Sexual Politics* (Garden City, 1970), 158. Inoue maintains that the patriarchal family system was necessary in wartime Japan as a prop for the imperial mythology. *Nihon joseishi*, 294.

⁹ Millett, *Sexual Politics*, 168.

helped cultivate within the existing family system.¹⁰ Farming employed 36 per cent of all married women. Better known than these peasant millions were the 435,800 girls aged twelve to twenty who worked as "indentured laborers" in manufacturing, living in company dormitories under strict regulation. However severe their living conditions, they comprised nevertheless only a minority of the 1.4 million women employed in factories. Commerce also used large numbers of women, mainly as assistants in small family enterprises. The professions, by contrast, were overwhelmingly male: only 3.7 per cent of working women held governmental or professional positions. "Labor outside the home was limited largely to the single";¹¹ married women earned money mainly in agriculture, home handicrafts, or small-scale commerce.

By 1940 there had been great industrial expansion in Japan but no break in the female pattern of working away from home only before marriage. Although the figures conceal the crucial distinction between full-time and part-time employment, it is evident that farming remained the chief occupation for women and that 87 per cent of all married women who worked were still engaged in agriculture or trade (74 and 13 per cent, respectively). Jobs open to women past age twenty-five were usually clerical or menial and rarely included the fringe benefits so essential for male labor harmony. This brought hardships to widows and divorcees as well as the unmarried, few of whom, moreover, could freely buy land to make a living from the soil because land was so scarce. In brief, as Irene B. Taeuber has written, "The major contribution of women remained the unpaid toil of the married women who assisted their husbands in field, shop, or house. Economic activity and social role were products of marital status rather than determinants of it." More bluntly put was the conclusion of Inoue Kiyoshi: most women of working age "pursued household labor as wife-slaves."¹²

Although wartime statistics from Japan are meager,¹³ it is clear that the number of males in the civilian labor force remained fairly high throughout the war, even after the fighting began in earnest in December 1941. Women constituted 35 per cent of nonmilitary workers in 1930, 39 per cent in 1940, and only 42 per cent in the census of February 22, 1944,¹⁴ despite the heavy military conscription of young men in 1942 and especially 1943. Although more than 95 per cent of all civilian males aged twenty to fifty-five were gainfully employed in 1944, barely half the women of comparable ages were listed as working. Nearly 40 per cent of all working women in that year were aged fifteen to twenty-four—the age range traditionally considered

¹⁰ Inoue, *Nihon joseishi*, 277–78.

¹¹ Irene B. Taeuber, *The Population of Japan* (Princeton, 1958), 115–16.

¹² *Ibid.*, 117–18, 218; Inoue, *Nihon joseishi*, 277.

¹³ On interpreting official figures, see Jerome B. Cohen, *Japan's Economy in War and Reconstruction* (Minneapolis, 1949), xi–xii, 275–76.

¹⁴ *Ibid.*, 290. Patterns of female employment in wartime Japan are also discussed in Thomas R. H. Havens, "Frontiers of Japanese Social History During World War II," *Shakai kagaku tōkyū* (Social Science Review), 1973, no. 52, pp. 28–29.

the appropriate time for girls to take jobs before marriage removed them from the labor force.¹⁵ Overall, between 1940 and 1944 the civilian work force lost 300,000 men and gained only 1.4 million women, including female students.¹⁶ Although the total number of women employed in industry doubled during these years, the war plants continued to prefer male labor: 75 per cent of workers in manufacturing and construction were male in 1930 and 76 per cent in 1944.¹⁷ Whatever the success of the female labor enrollment drives late in the war, the Japanese clearly relied on something less than a truly massive influx of new women workers to sustain their war economy.

Much greater changes, by contrast, took place in female work patterns among most of the major Western nations involved in World War II. More than six million American women took jobs during the war, raising the total number of working women by 50 per cent.¹⁸ In contrast, the female Japanese labor force rose less than 10 per cent between 1940 and 1944, although the prewar statistical base for the two countries is not precisely the same. In the Soviet Union the proportion of women in the civilian work force climbed from 38 per cent in 1940 to 53 per cent just two years later,¹⁹ mainly as a result of Operation Barbarossa. Labor conscription was especially systematic in Great Britain, where women between the ages of twenty and fifty were made liable to muster for civilian war service. Fully 2.2 million of the 2.8 million new British workers during the war were women. As Wright has put it, "No other warring nation (except perhaps the Soviet Union) imposed so great a burden on its female population."²⁰ Even in Germany, despite Hitler's legendary preference for keeping women in the kitchen and despite the desultory worker enlistment programs of his deputies, the proportion of women in the general civilian labor force rose from 37.4 per cent in 1939 to 52.5 per cent in 1944²¹—about the same rate of increase as in the USSR and several times the rate in Japan. Manufacturing employment in particular emphasized this contrast between Japan and Germany. Whereas the percentage of women in the wartime Japanese industrial work force actually declined, in Germany the proportion rose from 25.2 per cent in 1939 to 35.6 per cent in 1944.²² These figures are mere

¹⁵ Taeuber, *Population of Japan*, 337.

¹⁶ United States Strategic Bombing Survey, *The Effects of Strategic Bombing on Japan's War Economy* (Washington, 1946), 31.

¹⁷ Inoue, *Nihon joseishi*, 292; Cohen, *Japan's Economy*, 290-92. The percentage dropoff of women workers in manufacturing and construction was due partly to the collapse of the textile industry. In 1929, 83.9 per cent of all women working in industry were engaged in spinning, a figure that fell to 20.4 per cent by 1944. Inoue, *Nihon joseishi*, 292.

¹⁸ Chafe, *American Woman*, 135.

¹⁹ Wright, *Ordeal of Total War*, 59.

²⁰ *Ibid.*, 51. On women workers in England, see also Richard M. Titmuss, *Problems of Social Policy* (London, 1950).

²¹ Computed from figures in Cohen, *Japan's Economy*, 290.

²² Computed from figures in Alan S. Milward, *The German Economy at War* (London, 1965), 47. The jump in the percentage of working female Germans stemmed in large part from a drop

samples of the very detailed statistics on the employment of women in the various countries at war, but they certainly underscore the relative persistence of prewar female work trends in Japan compared with her allies and enemies.

It is tempting, but much too simple, to blame patriarchal officials for the relatively modest rise in the employment of Japanese women during the war. More to the point was the state's reluctance to conscript workers of either sex for the munitions factories. It relied primarily on reassignments from nonessential industries, bonuses and other voluntary inducements, and the mobilizing of students and older men.²³ Altogether approximately 1.5 million men were conscripted for civilian labor between 1939 and 1945,²⁴ comprising only eight per cent of the total male work force. These figures by themselves do not reveal the manpower deficiencies that plagued Japan during the period, but they are a sign that during most of the war the government's labor policy was based on the presumption of drawing more flies with honey than with vinegar. In a context of relative *laissez faire*, rather modest numbers of women took jobs outside the occupations traditionally open to them, despite the lure of high wages and the whip of income lost because working-age men were absent from the family in military service.

THE JAPANESE GOVERNMENT enlisted unmarried women for factory service in two stages, through random exhortation until late 1943 and by systematic inducement thereafter. General labor conscription became widespread in November 1941 under the national registration system (*Kokumin Tōroku Seido*), whereby men aged sixteen to forty and unmarried women between sixteen and twenty-five had to enroll, forming a potential labor supply. But only the men were actually drafted for factory work, via the familiar white summons that was only slightly less foreboding than the red document used for induction into the army. Other men and many schoolchildren were organized into patriotic labor associations (*Kinrō Hōkokutai*), starting in November 1941, for occasional volunteer work on "vital" projects.²⁵

Still, before 1943 there was little urgency about recruiting women workers since the nation's labor needs were easily met during the first year of the Pacific War—partly because the government expected a brief war and was tardy in expanding its munitions industries. In February 1942 the minister of welfare, Koizumi Chikahiko, disclosed the cabinet's attitude: "In order to secure its labor force, the enemy is drafting women, but in Japan, out

in the number of men remaining in the nonmilitary work force rather than from a significant rise, voluntary or otherwise, in the employment of women.

²³ Cohen, *Japan's Economy*, 271–72.

²⁴ Strategic Bombing Survey, *Effects of Strategic Bombing*, 31; Cohen, *Japan's Economy*, 317.

²⁵ Fujiwara Akira, Imai Seiichi, and Ōe Shinobu, eds., *Kindai Nihonshi no kisō chishiki* (Basic Knowledge about Modern Japanese History) (Tokyo, 1972), 470; Cohen, *Japan's Economy*, 387.

of consideration for the family system, we will not draft them.”²⁶ Tōjō Hideki, the prime minister, spoke somewhat more lyrically on the subject: “That warm fountainhead which protects the household, assumes responsibility for rearing children, and causes women, children, brothers, and sisters to act as support for the front lines is based on the family system. This is the natural mission of the women in our empire and must be preserved far into the future.”²⁷ Women could best serve the country, the government thought, by staying home, keeping their families happy, and producing more future citizens. Late in the war the Tōjō cabinet recognized the principle of registering all women—even married ones—but shirked a full-scale draft, relying instead on the “spontaneous” support of nominally volunteer women’s groups.

Although these traditional attitudes toward female labor predominated throughout the war, public thinking on the question was not unanimous. In late September 1942 the chairman of a prefectural assembly, Sakaguchi Takenosuke, told the Imperial Rule Assistance Association to use more women as agricultural technicians because the war meant that “the leaders and technicians in production, landscape gardening, animal husbandry, and every other specialty are insufficient and we are really in a fix. . . . I honestly believe we have no alternative but to encourage this activity by women.” Sakaguchi pressed beyond reasons of mere economic necessity to urge a greater self-awareness for women through education.

We have adhered unconsciously for too long to the ancient Japanese female virtue which says that men lead and women follow. Their natural spirit of autonomy and self-management has been suppressed, and their creative powers have been regarded as mere desperation. Times have changed, and today, when conditions are stringent, the most urgent business for women’s education is to encourage activity by women and to heighten their self-respect. It is not very simple to heighten self-respect, but the first thing to be done is to give young women the ability to believe in themselves. There is no way to acquire this ability except to give young women sufficient amounts of systematic practical education in school.

He concluded that Japan must “create female technicians” who “at the same time they help the men will raise the place of women and lead both women’s groups and the farm villages.”²⁸

Less progressive and more representative of popular attitudes was the diplomatic but pointed response of another male local leader, Nakamuda Shinroku, who observed at the same meeting that

the need for women’s education in wartime was fervently stated two days ago [by Sakaguchi], and I am truly in agreement. I want to scrutinize the training

²⁶ Koizumi Chikahiko, speech to the Diet, Feb. 1942, quoted in Yoneda Sayoko, *Kindai Nihon joseishi* (History of Modern Japanese Women), 2 (Tokyo, 1972): 66.

²⁷ Tōjō Hideki, quoted in *ibid.* See Inoue, *Nihon joseishi*, 294.

²⁸ Sakaguchi Takenosuke, speech of Sept. 1942, quoted in Taisei Yokusankai, ed., *Kokumin sōjōkaishi* (Record of National General Meeting) (Tokyo, 1942), 97, 527.

of women from another angle. . . . Today's young women will become mothers, and I wonder if even their children will not have to endeavor to finish up this great war? If so, women must cultivate, in a spirit of refinement and modesty, such characteristics as simplicity and vigor. This is especially true for girls' schools.

Woman's place was in the home, Nakamura clearly thought, even if she was educated.²⁹

By early 1943 the war had bogged down, the economy was showing signs of strain, and there was a labor shortage. Nevertheless the labor mobilization plan issued by the cabinet in spring 1943 still merely "urged" women to work in industry.³⁰ In July the patriotic labor associations were formalized and broadened, but many of the unmarried women who were mobilized through these units apparently fulfilled their service through soft office jobs or managed to evade it entirely.³¹ In September 1943 the government designated seventeen industries in which women workers should replace men on a noncompulsory basis, declaring that the most suitable women for this duty were those who had completed school, those whose schooling could be deferred, those who could be transferred from nonessential industries, and other single women aged fourteen and older. In Tokyo, for example, women's youth groups (*Joshi Seinendan*) were used to form seven patriotic labor associations, four in electrical manufacturing and one each in textiles, shoes, and pharmaceuticals.³²

Despite such steps the government remained ambivalent about the place of female labor in the war economy. By late 1943 women who were not working were being criticized as "women of leisure" (*yūkan josei*) or as "unpatriotic" (*hikokumin*), but Tōjō's idyl of Japanese womanhood by the hearth seemingly still underlay the state's continuing policy of "using and discarding" (*tsukaisute*) the labor of women. Rather than improving conditions for those who wanted to go on working, the government, as well as big industry, regarded the woman as temporary help, someone who should return home as soon as she was married. As a high officer in the ministry of welfare explained to another meeting of local leaders convened by the Imperial Rule Assistance Association in July 1943, "To draft women requires a great many facilities and labor management for females, but because we have not made arrangements of that sort, we will not carry out [the conscription of women]." The Greater Japan Women's Association, echoing official policy, declared in the same month that women could best serve the nation by engaging in handicrafts and side industries at home.³³ The

²⁹ Nakamura Shinroku, speech of Sept. 1942, quoted in *ibid.*, 315.

³⁰ Cohen, *Japan's Economy*, 315.

³¹ See Fujiwara, Imai, and Ōe, *Kindai Nihonshi no kisō chishiki*, 471.

³² Rekishigaku Kenkyūkai, ed., *Taiheiyō sensōshi* (History of the Pacific War), 5 (Tokyo, 1973): 121; Nihon Seinenkan, ed., *Dai Nihon Seishōnendanshi* (History of the Greater Japan Youth and Children's Associations) (Tokyo, 1970), 654-66.

³³ Quotation in Yoneda, *Kindai Nihon joseishi*, 2: 67; see also 2: 64-68.

state obviously wanted women to work hard in the factories before marriage but to stay home thereafter, however useful their labor might have been once they gained more experience on the job.

The second phase of enlisting women workers began on January 23, 1944, with Tōjō's announcement to the lower house of the Diet that women's volunteer labor corps (Joshi Rōdō Teishintai) were to be created for work in aircraft manufacturing and other essential industries. The volunteer corps were much more centralized and efficiently managed than the patriotic labor associations, which they now displaced. A new registration of unmarried women aged twelve to thirty-nine was conducted in February, and neighborhood association leaders hounded eligible women to join the corps. Their period of service was no longer to be just a few weeks but a full year, and it was later extended to two years.³⁴ By March 1945, according to ministry of labor figures, 472,000 women had gone to work through the volunteer corps, although the ministry of welfare estimated that half had been working elsewhere before.³⁵

Although it was difficult for single women to refuse to serve in the volunteer corps, the new scheme fell short of being a compulsory labor system. The national mobilization plan for the spring of 1944 provided for these corps but cautioned that "due consideration shall be given to the limitations of women."³⁶ In practice women who were badly needed at home were often excused, and at all times single women were encouraged to marry, matrimony immediately releasing them from further service. In spite of such prevailing societal values, the state labor enrollment program functioned rather efficiently, considering that it was confined by statute to the unmarried minority of adult females. Nearly ninety per cent of the 5.4 million women who registered in February 1944 were already working. Nevertheless, once the volunteer corps were operating and another enrollment had been conducted in November 1944, slightly fewer than 5.7 million women were enlisted, but the number of registrants not working had dropped only slightly, from 592,000 to 486,000.³⁷ Presumably there were undetermined numbers of others who continued to evade registration entirely.

Even before these figures had been assembled the perils of underutilizing women workers were made plain in an inventory of human resources issued by the top war leadership on August 11, 1944. This document, *Teikoku kokuryoku no genjō* (Current Condition of the Empire's Strength), pointed out that male labor reserves were slim but that there was still a huge supply of potential female workers.³⁸ In the same month the cabinet adopted a

³⁴ Nihon Seinenkan, *Dai Nihon Seishōnendanshi*, 780-87; Cohen, *Japan's Economy*, 317; Fujiwara, Imai, and Ōe, *Kindai Nihonshi no kisō chishiki*, 471.

³⁵ Rōdōshō, ed., *Rōdō gyōseishi* (History of Labor Administration), 1, quoted in Rekishigaku Kenkyūkai, *Taiheiyō sensōshi*, 5: 121; Strategic Bombing Survey, *Effects of Strategic Bombing*, 31; Cohen, *Japan's Economy*, 321.

³⁶ Quoted in Cohen, *Japan's Economy*, 315.

³⁷ Tacuber, *Population of Japan*, 340.

³⁸ *Teikoku kokuryoku no genjō*, quoted in Konishi Shirō, "Senjika no kokumin seikatsu"



Fig. 2. Japanese women university students performing wartime factory labor service. Photograph courtesy Asahi Shinbunsha and Dōin Gakuto Engokai.

stiff law providing a fine of up to one thousand yen (about five hundred dollars) and imprisonment for up to one year for women who refused to enroll in volunteer corps,³⁹ but it is obvious from the large number of women who never served, some of whom never even enrolled, that these sanctions were rarely enforced. Finally in October 1944 the nearly four million women who were working in the war industries were frozen in their jobs by state decree. This action was the last major step taken by the government to mobilize women. The labor pinch had already eased in late summer; thereafter matériel shortages led to a slowdown in heavy industry. After large-scale bombing raids by the United States began in November

(People's Livelihoods in Wartime), in Morimatsu Yoshiaki *et al.*, eds., *Taikei Nihonshi sōsho* (Series on Japanese History), 17 (Tokyo, 1969): 448.

³⁹ Rekishigaku Kenkyūkai, *Taiheiyō sensōshi*, 5: 121.

and people started to flee the cities in great numbers, the state quite simply lost control over the movement of workers in its war plants.

FROM ACCOUNTS WRITTEN BY WOMEN in the volunteer corps, life in the war industries was grim indeed. However severe toil in prewar spinning mills had been for the thousands of girls who were recruited from the countryside by silver-tongued hiring agents, wartime factory work for the young women volunteers was probably even more unpleasant, if only because of the hardships imposed by war. Nakamoto Hiroko, whose school in Hiroshima ceased to function in the fall of 1943, was put to work in an airplane factory under conditions common to most women who worked in the war plants. The night shift was obliged to sleep in the factory, which was plunged into cold by the utter lack of heating fuel. By 1944 she and her fellow workers received for lunch only a bowl of broth with a few noodles. Nakamoto lacked much involvement with her job since she was never told exactly what the steel she checked with micrometers was to be used for. Her incentive dropped even further, she recalled, when equipment and parts grew scarce and she was forced to stand idly for many hours each day without being permitted to read.⁴⁰

For those who lived in company dormitories, fatigue and filth were especially demoralizing. Urabe Takeyo, a housemother in a factory lodge for twenty-five teen-age girl workers near the campus of Keio University in Tokyo, noted how exhausting the nightly air raids became when the great American bombardments began in late 1944. Living so close to a noisy plant made sleep fitful at best, and the bombings meant that rest was nearly impossible. What was more, smoke from the coarse fuel used by the factory coated the surroundings: "The entire neighborhood seemed black from oil." Urabe, in charge of having the girls' work uniforms laundered, found that the staple fiber (*sufu*) from which they were made soon tore. Near the end of the war, she reported, clothes and bedding could no longer stand washing, and food was so scarce that only the ubiquitous lice grew fat.⁴¹

One of the best-known women's volunteer corps was the Toyokawa Joshi Teishintai, formed in the summer of 1944 in Ishikawa prefecture for service at the mammoth Toyokawa naval arsenal on the northern outskirts of Toyohashi, in Aichi prefecture. Joining the Toyokawa Joshi Teishintai was "virtually compulsory" for girls unlucky enough not to have been previously placed in safer jobs by their parents. Many who joined were maids, waitresses, and entertainers, and the rest were unemployed girls living at home. Later accounts noted that many were resentful at going off to hard

⁴⁰ Hiroko Nakamoto, as told to Mildred Mastin Pace, *My Japan, 1930-1951* (New York, 1970), 48-51.

⁴¹ Urabe Takeyo, "Shirami" (Lice), in Izumi no Kai, ed., *Shufu no sensō taikenki* (Record of Women's War Experiences) (Nagoya, 1965), 144, 146. On the prevalence of lice in wartime England, see Titmuss, *Problems of Social Policy*, especially 126-31.

labor in a far-off city, especially if they had never been away from home. Koma Keiko wrote concerning her departure for Toyokawa: "Weren't we heroic" to go off to an unknown city to work? "Then I realized my folly." Yet there were some among the first contingent in early September 1944 who were "courageous" and eager to serve. One such young woman, a former liquor-store clerk who was inured to surreptitious outlays of spirits to policemen, politicians, and tax officials in exchange for favors, said she refused to let her father bribe her way out of the volunteer corps because she was anxious to join the war effort.⁴²

Despite a daily regimen that began at 5:30 A.M., the Toyokawa volunteers enjoyed good facilities and apparently lived in more comfort than most other wartime laborers. They worked from 7:30 A.M. to 5:30 or 6:00 P.M., but they were otherwise quite free for those small pleasures shared by younger teen-age girls in sheltered dormitories everywhere: letter writing, music lessons, and weekly dramatic skits.⁴³ Such tranquillity ended suddenly in January 1945, when the arsenal workers were evacuated to subcenters in Nagano, Yamanashi, and Mie prefectures to avoid the American air raids. Most members of the volunteer corps were included in the dispersal.

Although the workers were happy to escape the threat of bombing, hunger quickly depressed the spirits of the volunteers who fled to Iida and Matsumoto in Nagano prefecture. Staying behind in Toyokawa was even worse: there was constant worry about the bombings and fewer persons with whom to share it. Some volunteers simply vanished; others drifted back to Ishikawa to marry. Yet there were also those who showed greater determination and vowed "to stick it out." On the evening of August 7, 1945, U.S. airplanes demolished the arsenal, killing 2,445 persons, all but 130 of them civilians, and fatally wounding thirty-three others. Of the ten volunteers in lodge number twelve who were still assigned to Toyokawa, nine died and one miraculously escaped.⁴⁴

JAPAN'S LABOR NEEDS during World War II were satisfied primarily by redeploying male workers from nonessential industries. This basic supply was augmented by five million women (mostly unmarried), three million students, more than one million older men, hundreds of thousands of imported Chinese and Korean laborers, and, in the last year of the war, more than two million men deferred from military service.⁴⁵ Women not only played a numerically modest part in the war economy but were also paid

⁴² Tsuji Toyoji, ed., *Aa Toyokawa Joshi Teishintai* (Toyokawa Women's Volunteer Corps) (Tokyo, 1963), 13-16, 203, 46, 86, 47-49. The Toyokawa arsenal, situated near the local Inari shrine, was hastily built after the outbreak of war with China and quickly became the largest naval ordnance depot in Japan.

⁴³ *Ibid.*, 93-113.

⁴⁴ *Ibid.*, 178-84, 186-93, 203, 232.

⁴⁵ Strategic Bombing Survey, *Effects of Strategic Bombing*, 31; Cohen, *Japan's Economy*, 316-24.

less for their work than were men in similar jobs. Wages continued to be scaled according to the familiar prewar criteria of age, sex, and experience rather than work performed.⁴⁶ As in America, child and female labor laws were set aside or ignored from 1943 on, although in the last year of the war the average daily work hours for men and women remained below the legal maximum of twelve.⁴⁷

Those women who held jobs entered the work force in patterns, as Jerome B. Cohen noted in 1949, that reflected the persistence of male privilege.⁴⁸ Despite legal restrictions after December 1941 on changing occupations, about 650,000 women were drawn to urban employment from the countryside between July 1937 and February 1944, according to the ministry of agriculture and forestry.⁴⁹ Nevertheless, even though overall employment in agriculture remained constant between 1940 and 1944, the proportion of cultivators who were women rose from 52.2 to 57.6 per cent.⁵⁰ In the urban sector, just as the munitions industries did not welcome women en masse, there was no great influx of women into professional or government-service jobs: the number of professionals and bureaucrats swelled from 2.1 million in 1940 to 2.9 million in 1944, yet the percentage of women in these categories rose only slightly, from 31 to 34.7.⁵¹

Clearly, despite the effects of the military draft, significant numbers of women were not brought into important positions of executive or production-line responsibility. Instead most of them worked as before in the field, the market place, the kitchen, the stock room—wherever low-paid light labor was needed. Women had actually outnumbered men in the factories before 1930, owing to spinning-mill jobs, but now they represented less than a quarter of the industrial work force. Another sign that women were less than fully mobilized for war work was the continuing presence of domestic servants, nearly all of whom were females. Although there were 230,000 fewer servants in February 1944 than three and one-half years earlier, there were still enough well-to-do families to provide employment for 600,000 domestics.⁵²

After the war the ministry of welfare dismissed the country's reluctance to use women except at the lowest levels of the economy as conservatism on the part of the wartime elite. Women were never conscripted, a report

⁴⁶ Cohen, *Japan's Economy*, 327–28. For the continuing inequalities in women's labor in the United States, see Chafe, *American Woman*, 151–73.

⁴⁷ Polenberg, *War and Society*, 146; Cohen, *Japan's Economy*, 341.

⁴⁸ On women workers in wartime, see Cohen, *Japan's Economy*, 273–74, 290–93, 316–22, 341; and Havens, "Frontiers of Japanese Social History," 29–30.

⁴⁹ Bruce F. Johnston, with Mosaburo Hosoda and Yoshio Kusumi, *Japanese Food Management in World War II* (Stanford, 1953), 94–95.

⁵⁰ Takekazu Ogura, ed., *Agricultural Development in Modern Japan* (2d ed.; Tokyo, 1967).
⁵² Johnston gives the percentage in 1944 as 58.4. *Japanese Food Management*, 97. On rural women, see Ide Fusae and Nagahara Kazuko, eds., *Nōson fujin mondai bunken mokuroku* (Bibliography on the Problem of Rural Women) (Tokyo, 1952).

⁵¹ Cohen, *Japan's Economy*, 293.

⁵² *Ibid.*, 292.

claimed, because "the upper class with prejudice against factory labor did not support the public opinion that women also should be conscripted to war production."⁵³ Perhaps the prejudice was against women as well, for it seems almost certain that public opinion—male and female alike—was no more favorable than that of the wartime leaders toward compulsory women's labor. It is true, of course, that women often lacked technical ability for industrial occupations, but the crux of the matter was that "traditional ideas of appropriate activity for women remained economic deterrents throughout the war years."⁵⁴

Time-honored values within the culture undoubtedly constrained the state's capacity to put women to work on behalf of the war. Yet in the absence of so many men, economic necessity forced more women to work than ever before, mocking the government's policy of keeping them at home. Despite the state's unwillingness to meddle too extensively with existing male-female economic relationships, the war made wage earners of many women for the first time and as a result caused changes in their roles within the family that would have been difficult to imagine a decade earlier.

SOMEWHAT LESS EVIDENT than the effect of the war on employment patterns is its impact on marriage and fertility. War obviously has an unsettling effect on family life and fertility by separating couples for long periods of time, and it also invites changes in sexual mores, socially imposed feminine roles, and the incidence of venereal disease. Data from Japan generally confirm the view that modern warfare has surprisingly slight demographic effects in the long run.⁵⁵ There is a great deal that needs to be clarified about male-female relationships in wartime Japan, but it is apparent that the authorities endorsed the idea of family solidarity and urged women to stick to their prewar social norms.

The women's youth groups, for example, began in the spring of 1941 to operate government-supported marriage counseling centers designed "to cause women to move from an individualistic view of marriage to a national one and to make young women recognize motherhood as the national destiny."⁵⁶ The ensuing marriage-improvement movement was linked with Tōjō's new fertility campaign. For many years Japan had told the world she needed more territory for her crowded population. Once the war to secure that living space broke out, the government suddenly decided the

⁵³ Ministry of welfare, "Women Labor in Industry," Tokyo, Dec. 18, 1945, p. 7, quoted in *ibid.*, 320.

⁵⁴ Tacuber, *Population of Japan*, 337.

⁵⁵ See Polenberg, *War and Society*, 146–53. On the relationship between modern war and demography, see Frank B. Livingstone, "The Effects of Warfare on the Biology of the Human Species," in Fried, Harris, and Murphy, *War: The Anthropology of Armed Conflict and Aggression*, 3–15.

⁵⁶ Nihon Seinenkan, *Dai Nihon Seishōnendanshi*, 314. The text speaks of a "state" or "national" race (*kokka minzoku*) and of a "statist" or "national" (*kokkateki*) destiny.

country was shorthanded and passed a national eugenics law to promote childbearing. Cabinet officials were alarmed that the live birth rate had dropped from thirteen per thousand before 1937 to less than ten per thousand in wartime, while the number of insane was rising. A new law, accordingly, outlawed birth control and required that the insane be sterilized.

Not content with these negative steps, the government also revealed plans to raise birth rates by nearly one-half. If all women cooperated, it declared, the target of three million births each year could easily be reached. From her comfortable Tokyo home in fashionable Daizawa, Tōjō Katsuko, the wife of the prime minister and the mother of seven children, announced cheerfully that "having babies is fun," and she told her fellow countrywomen to shirk all luxuries so that they could afford to raise large families. The state promoted early marriages, set up matchmaking agencies, and asked companies to pay baby bonuses to their workers. The government lent couples wedding clothes if they were too poor to afford a ceremony. Families with ten children or more were promised free higher education.⁵⁷

Despite these incentives, it is difficult to detect much change in the wartime birth rate. Between 1941 and 1943, when the state promoted its natalist policies most strongly, the marriage rate rose to roughly ten per thousand population, up from 8.1 per thousand during 1935-39. To some degree this increase reflected the cash and benefit inducements offered to soldiers for having legal wives, so that informal marriages, by custom not recorded until pregnancy or other exigency occurred, were promptly registered. More to the point, the fertility campaign took place amid economic expansion, when young men could afford to marry somewhat earlier. Births themselves, however, merely held steady at their 1940-41 level of about 2.2 million per annum through the year ending September 30, 1944. Then they plunged more than ten per cent during 1944-45 and another fifteen per cent during the following year. This downturn, a response to worsening conditions from mid-1943 onward, was linked to larger draft calls of potential fathers, internal migration to flee the bombings, economic deterioration, and lower levels of public health and nutrition that inhibited successful pregnancies.⁵⁸ A sad incidental development was that those babies who were born in 1942 were demonstrably smaller than children born just two years before. Presumably because of dietary insufficiencies during gestation, boys in 1942 averaged 1.8 centimeters shorter and 209.4 grams lighter than in 1940. For girls the figures were 2.3 centimeters shorter and 235.3 grams lighter. These children's prospects for making up the loss were clouded by

⁵⁷ Quotation in Joseph Newman, *Goodbye Japan* (New York, 1942), 238; see also 237-39. The natalist question is sketched in Havens, "Frontiers of Japanese Social History," 27-28.

⁵⁸ Tacuber, *Population of Japan*, 223, 355, 354.

the strict rations on milk products for infants that had been implemented on November 1, 1940.⁵⁹

Under the best of circumstances a state has only limited leverage over so private a matter as the reproductive propensities of its people, and it is small wonder that Japanese women failed to respond more enthusiastically to the government's blandishments in 1941-43 to produce more children. That such a policy existed at all, as Yoneda Sayoko has written, shows that the state regarded women not as a human resource like men, to be used for war production, but as a reproductive resource for future manpower.⁶⁰ This approach was only re-emphasized in 1942 in the government movement to love and protect children, a thinly disguised natalist program, and in the scheme of June 1943 to guard women's livelihoods by eliminating venereal disease, tuberculosis, and infant mortality.⁶¹

At the same time the war caused inevitable tensions in the relationships between men and women. The state, abetted by self-righteous matrons in the Greater Japan Women's Association and the neighborhood associations, closed down nearly all the geisha houses, brothels, and entertainment centers in the country, putting the displaced women to work in labor corps.⁶² More to be pitied, perhaps, were the schoolgirls who were forced to send "letters of encouragement" to soldiers they had never met, even when they received coarse replies from the front.⁶³ Still, in contrast to America, where there was a good deal of gossiping about the reputed profligacy of teen-age girls during wartime, conditions remained strict in most women's factory dormitories in Japan. Girls at the Toyokawa arsenal could receive male visitors only if they were blood relatives, and then only in a public parlor.⁶⁴

War affected the physiques as well as the emotions of working women in unfavorable ways. When Urabe Takeyo inexplicably began to miss her menstrual period after moving into her company dormitory in April 1943, she was given a hormone injection at the factory dispensary and the opaque explanation: "Wartime loss of menstruation, shall we call it? It is a mental thing that has to do with nutrition."⁶⁵ This phenomenon was reportedly quite common also among young women in the patriotic labor associations and women's volunteer corps. In a more general sense, the strain of factory work depressed many women workers. There was a good deal of emotional fatigue among the young women volunteers after they had spent several

⁵⁹ Tōyama Shigeki, Imai Seiichi, and Fujiwara Akira, *Shōwashi* (History of the Shōwa Era) (Tokyo, 1959), 224, 226.

⁶⁰ Yoneda, *Kindai Nihon joseishi*, 2: 67.

⁶¹ See Shimonaka, *Yokusan kokumin undōshi*, 1070; Nihon Seinenkan, *Dai Nihon Seishōn-endanishi*, 641.

⁶² Masuo Kato, *The Lost War: A Japanese Reporter's Inside Story* (New York, 1946), 12.

⁶³ Takaaki Aikawa, *Unwilling Patriot* (Tokyo, 1960), 74.

⁶⁴ See Polenberg, *War and Society*, 149-53; Tsuji, *Aa Toyokawa*, 135-40.

⁶⁵ Urabe, "Shirami," 145.

months at Toyokawa. Although a few were returned home after treatment at the arsenal hospital, most were simply sent back to work with admonitions such as "carry on for the sake of the country" or "there can be no apology to soldiers who have died honorably."⁶⁶

With so many men under arms toward the end of the war, relations between men and women were understandably disturbed. Not only did women often have to become breadwinners, but they also displaced men as the main authority figures for children in millions of families, with nearly incalculable effects on the children as well as on the women themselves. Tsurumi Kazuko has outlined the conflict of the wartime mother caught between the public duty of patriotism and the private sentiment of wanting her son home from battle. Likewise there was the paradox of the Japanese war widow, ideally venerated for her sacrifice but in reality often humiliated through ostracism like any other widow.⁶⁷

At its height the military draft removed more than seven million men from the civilian population, many of them posted overseas or at bases in the home islands distant from their families. Altogether 2.8 million Japanese, mainly in their twenties and thirties, died as a result of the war. Yet it is remarkable that the long-term fertility of the Japanese population was not adversely affected by wartime. The overall pattern of the 1940s, regardless of fluctuations early in the decade, was a continuation of the slightly downward fertility trend evident since World War I, and it is thought that Japan's population in 1950 was close to the level expected if war had never occurred.⁶⁸ In sum, neither Tōjō's natalist efforts in the early forties nor the ravages of bombing, malnutrition, and war deaths in the later months of the war were enough to alter the reproductive habits of Japanese society.

FOR WOMEN WAGE EARNERS and housewives alike, getting on with daily living during the war turned primarily on the age-old question of finding enough food, clothing, and shelter. Although the country never plunged into starvation or total lack of clothing and housing, consumer commodities grew scarce and had to be carefully allocated through a complex rationing and distribution system.

Women were subjected to controls on what they might wear shortly after war with China broke out. From 1939 on, they were discouraged from wearing cosmetics and having permanent waves; hairdressers were permitted to give only three curls to each customer, a restriction that must have taxed their ingenuity to the utmost. Patriotic women at street corners handed out literature from the national spiritual-mobilization movement to girls dressed in expensive clothing, asking greater sober-mindedness in light of the

⁶⁶ Tsuji, *Aa Toyokawa*, 117-19.

⁶⁷ Kazuko Tsurumi, *Social Change and the Individual* (Princeton, 1970), 258-63.

⁶⁸ Taeuber, *Population of Japan*, 340, 356, 358.

national emergency.⁶⁹ But the ostracism of gaudy dress and compulsion to wear *monpe*—drab pantaloons typically worn by farm women in north-eastern Japan—could not conceal the huge wartime shortages of textiles that prevailed by 1940.⁷⁰ Whatever their personal inclinations in response to such patriotic hectoring, therefore, most persons were obliged by the operation of market forces to economize on clothing and personal grooming. After Pearl Harbor true deprivations in food, clothing, and shelter probably made these moralistic exhortations from the spiritual-mobilization movement seem gratuitous and silly.

Once the war against the United States began, Japanese women expressed a great deal of concern over the policies for allocating clothing. On the one hand, village women's associations complained as early as September 1942 about the lack of fabric for diapers and for wrapping the stomachs of pregnant women to keep them warm. Because of this insufficiency, it was rather unscientifically claimed, an abnormally high rate of miscarriages and premature births existed—400,000 to 500,000 per year. On the other hand, the Greater Japan Women's Association pondered whether to make the *monpe* required for all. Yamadaka Shigeri, a female director of the association, attacked the ministry of welfare in September 1942 for permitting department stores to continue showing impractical, wasteful Western fashions rather than the cheap and useful *monpe*. At the local level, however, women's groups worried less about standardization than about practical matters of supply. One report noted that

when you go to restaurants and hotels, there seems to be quite a sufficient supply of towels. The people are at their wit's end with desire for such things as this whose distribution was [supposedly] banned and would like to receive some. They never say they want things that are lacking, but they would really like to have the surpluses that are available of the things they most need. These are matters that are always criticized at our regular meetings.⁷¹

In fact wearing the national uniform was never made obligatory for men or women, despite a cabinet directive in June 1943 that placed limits on having new clothes made and set guidelines for styles.⁷² Probably the government did not have to bother, because market pressures made it almost impossible to obtain or afford anything else. By 1945 kimonos were no longer seen, and new shoes, even wooden clogs (*geta*), were so scarce that the state put schoolchildren to work making straw sandals (*waraji*).⁷³

⁶⁹ Newman, *Goodbye Japan*, 236.

⁷⁰ Hashikawa Bunzō, Kano Masanao, and Hiraoka Toshio, eds., *Kindai Nihon shisōshi no kisō chishiki* (Basic Knowledge about the History of Modern Japanese Thought) (Tokyo, 1971), 457–58.

⁷¹ Taisei Yokusankai, *Kokumin sōjōkaishi*, 331, 613–14.

⁷² "Outline for Implementing Simplified Wartime Clothing Habits," June 1943. See Konishi, "Senjika no kokumin seikatsu," 442; and Kato, *Lost War*, 191–92. The men's *kokuminfuku*, or national civilian uniform, had been officially adopted in 1940, but men were never legally required to wear it.

⁷³ Kato, *Lost War*, 11–12.

Food was even more worrisome to Japanese women in wartime. Restrictions were combined with exhortations as early as 1939 to encourage restraint in the kitchen. In that year people were urged to content themselves with *hinomaru bentō*—box lunches in the pattern of the Japanese flag, a pickled red plum on a field of white rice—an economy that by 1944 seemed a luxury to civilians who could no longer obtain pure rice at all.⁷⁴ Suzuki Yoshiko, a rice distribution clerk, recalled that under the wartime rationing system women would storm and rage if their allotments were off by even a hundred grams. Others would appear two or three days before their family's next fourteen kilogram sack was due to be distributed, petulantly asking for help to tide them over. A favorite response at Suzuki's center was to tell the customer that she looked pregnant and to grant her the seventy gram supplement owed to expectant and nursing mothers.⁷⁵ Emblematic of the wartime food problems facing women was this unappetizing recipe from an outspoken housewife:

Let me introduce our teatime specialty, *nukapan* [rice bran fried in a pan]. Mix wheat flour and rice bran, add a little water, and fry in a pan. Add no eggs or sugar. Fry for two minutes. It looks just like good custard. But it tastes bitter, smells like horse dung, and makes you cry when you eat it.⁷⁶

Housing too became a nightmare, especially under the impress of air raids and the evacuation of at least ten million persons to the countryside, not to mention the deliberate destruction of block after block of city homes to create fire-breaks in the event of bombing.⁷⁷ Escape to the rural areas, far less systematically conducted than in Great Britain, was not only psychically disorienting but also involved crowding in the village farmhouses that took in the evacuees, two-thirds of whom were women. Yet the U.S. Strategic Bombing Survey reported that during the war women of all ages had higher morale than men, a finding attributed to women's traditional training in obedience, their lack of education, and their inexperience in adapting to pressure. The survey also presumed that men's outlooks were more sober because they had more to lose from defeat. Men "apparently had the background to interpret far-off defeats, whereas more of the old, the ignorant, and the women needed a cascade of fire bombs to awaken them" to impending defeat.⁷⁸ These conclusions are questionable not only because of the highly artificial milieu of defeat and suspicion surrounding the inter-

⁷⁴ Interview with Kazuko O. Amemiya, Tokyo, May 9, 1973.

⁷⁵ Suzuki Yoshiko, "Kiga no omoi" (Thoughts about Hunger), in Izumi no Kai, *Shufu no sensō taikenki*, 154–55.

⁷⁶ Quoted in Fujiwara, Imai, and Ōe, *Kindai Nihon shisōshi no kisō chishiki*, 458.

⁷⁷ Cf. the graphic film *Sokai* (Evacuation), first shown in July 1944 to encourage dispersal to the countryside, shown at the Film Center, Kyōbashi, Tokyo, March 23, 1973. It is summarized in *FC fuirumu sentā* (FC Film Center), no. 13, Mar. 8, 1973, p. 20.

⁷⁸ United States Strategic Bombing Survey, *The Effects of Strategic Bombing on Japanese Morale* (Washington, 1947), 61, 224. On the reception of evacuees in rural Britain in September 1939, see Titmuss, *Problems of Social Policy*, 110–36.

views conducted in autumn 1945 upon which they are based. They also seem flawed by an underemphasis on the gap in education between men and women and by the fact that proportionately more women were living in rural districts at all stages of the war, cut off from the worst and most visible signs of damage and deprivation. Schooling and residence, not sex, seem to have been decisive in shaping people's outlooks on the likelihood of victory or defeat.

WHAT IMPLICATIONS DO WOMEN'S EXPERIENCES in Japan disclose about war and the history of society? When mobilization, patriarchy, and wartime hardships interacted, what were the consequences? For one thing, it is clear that the state sanctified motherhood, encouraged traditional supportive activities by home-front women's groups, and avoided to the end fully mobilizing the labor of women by law. The government relaxed its priority on male precedence rather slowly under the weight of a deteriorating war situation, and then with little effect, despite the impressive numbers of unmarried women mustered late in the war. As elsewhere, war was a time for rulers to reaffirm accepted symbols and conventions, not a time for redefining them.

It cannot be inferred, moreover, that women suffered from the war more than other social groups, except in the crudest numerical sense that there were more of them. Most civilians managed to get by during wartime, and the records they left behind are scant compared with the striking testimony of those whose agony was greatest, most notably the Hiroshima victims. The years from 1937 to 1945 were trying, and sometimes devastating, for nearly everyone in Japan, but predictable habits proved to be very durable and the basic integrity of Japanese society was not endangered by wartime conditions. No doubt these facts derive partly from the long-standing customary muting of adversary relationships among the Japanese people, to a degree rarely found in other societies. Like labor-management, plaintiff-defendant, and most other relations in Japan, male-female interaction during the war continued to avoid competition and conflict, assuring social harmony but compromising ultimate producing efficiency as it is understood in the West. Whether heightened efficiency in objective terms would have made much difference to Japan's war effort seems doubtful, but it would certainly have entailed high costs in terms of social frictions that the Japanese were unwilling to bear.

The temporary dislocations in a wartime society such as Japan's are much easier to identify than the long-range effects. Estimating the precise impact of World War II on Japan is complicated by two abnormal postwar factors: first, the military occupation by a conquering nation for six and one-half years, leading to more rapid social reforms than might otherwise have been the case; and second, the extraordinarily fast economic development after

1952, which probably changed people's lives more dramatically than the social engineering of the occupation. Although Japan's encounter with total war led to total defeat, the wartime experience was a vital hinge on which the history of her modern society turned. However daunting the traditional Japanese ideas of male supremacy continued to be during 1937-45, the specifics of a war economy undercut the patriarchal ideology in ways hard to imagine without warfare. The unavoidable need to work or rear children alone, brought about by exigencies initiated perhaps by the state but now beyond its control, meant that life for Japanese women would never be the same again. As a social institution, war has effects on the history of peoples possibly beyond measure. While it led ineluctably to victory and defeat on the battlefield, World War II exposed winners and losers alike to social consequences scarcely foreseen when Hitler and Tōjō dispatched their forces to Poland and Pearl Harbor.

Reviews of Books

The loss of Mr. Appleby and several resignations in the past year have added a heavy burden to the editorial staff. Currently we have an excessively large number of books on hand that have not been processed for reviewing, and we have only a limited amount of space in the AHR for the printing of reviews. For this reason it will be necessary to reduce the number of words allocated to each review until we can again be current. Our handling of reviews has been further hindered by the failure of some of our reviewers to meet their deadlines. We ask that, during this difficult period of reducing our backlog, our reviewers be prompt and our readers patient. We greatly appreciate your cooperation.

GENERAL

A. E. J. MORRIS. *History of Urban Form: Prehistory to the Renaissance*. (A Halsted Press Book.) New York: John Wiley and Sons. [1974.] Pp. x, 268. \$19.75.

Mr. Morris offers a work perhaps more useful for reference than for continuous reading. He does, indeed, have a general thesis, namely that down to the seventeenth century there were two principal urban patterns: organic and irregular, and orthogonally planned. Cities that developed naturally followed the dictates of terrain or human needs. Cities that were deliberately founded or rebuilt show rectangular or square blocks separated by straight streets. From the beginning planned cities presented the difficulty of fitting an orthogonal design into a circular defensive wall without waste of space; a circular wall was the most economical to construct and hence the commonest form. For late examples may be cited Naarden in Holland (p. 113) or Neuf Brisach in France (pp. 160-61). Only from the seventeenth century were occasional experiments made with radial plans

within a polyhedral enclosure, e.g., Palma Nova in Italy (pp. 117-18). The most interesting city plans were those which imposed regular design on all or parts of existing, organic cities, generally but not necessarily capitals: for example, Rome as redone by Sixtus V and later designers; Paris, especially from Louis XIV through Napoleon III and Haussmann (pp. 138-48, 162); or London, particularly in the rebuilding after the Great Fire of 1665 (pp. 181-205).

This general thesis, however, does not provide adequate continuity and unity to what is essentially a series of discussions of the design growth of many individual cities. Morris deals briefly with the ancient Middle and Near East, from Sumer in the third millennium B.C. and including both Egypt and the Harappan cities of the Indus Valley. He treats more fully Greece, Rome, and medieval Europe. His main emphasis is, however, on the Renaissance in Europe, a period which he extends through the eighteenth century and on occasion into the nineteenth. He holds that the sudden expansion of cities which resulted from the industrial revolution introduced such radical changes into matters of urban design that the modern period represents, to use a colloquial phrase, "a different ball game." Renaissance Europe is considered by countries: Italy, France, the rest of Europe (including Eastern Europe and western Russia), and Britain. A final chapter considers "Urban USA" down to the Civil War. Other parts of the world are cursorily presented in a series of appendixes.

The book is fully illustrated with over three hundred plans and photographs. The plans, generally in the page margins, are small but adequate. Also in the page margins are relevant quotations from earlier writers; it may be felt that these, though pleasant, do not illuminate the treatment of the given city. The book concludes with a short bibliography by chapters and two indexes, of place names and general.

Mr. Morris's book is perhaps the most recent of a number of histories of urban design. Of these may be cited here particularly the series of individual short books in *Planning and Cities*, a series published by Braziller in New York. These latter volumes are for the periods or topics covered fuller, more unified, and better illustrated—both with larger plans and more photographs. Morris's history has the advantage of compacting into one volume the whole sweep of urban design from Sumer to the opening of the nineteenth century.

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GUENTER LEWY. *Religion and Revolution*. New York: Oxford University Press. 1974. Pp. xvii, 694. \$17.50.

DONALD EUGENE SMITH, editor. *Religion and Political Modernization*. New Haven: Yale University Press. 1974. Pp. x, 340. \$15.00.

The Chinese *Li Chi* or *Book of Rites* says that "of all the ways of keeping men in good order, there is none more important than sacrifice." The Greek historian Polybius noted that "a scrupulous fear of the gods is the very thing which keeps the Roman commonwealth together." For Machiavelli religion was "the most necessary and assured support of any civil society." In our time Peter Berger says that "there can be little doubt about the overwhelmingly conservative and inhibitory effect of religion in most periods of history."

Guenter Lewy knows all that. So do Donald E. Smith's symposiasts. Yet both Lewy and Smith also document alternatives to such static views. The reader may need a compass, an atlas, and the spirit of the free-fall diver to keep the pace of the two books; both have astonishing scope. The chapters of Smith's book were first read as papers at a Honolulu conference in 1971 by recognized specialists on the many cultures covered. Lewy occasionally has less command of his material than do they—no one could be truly expert on all the places and times he treats—but his achievement, dependent though it often had to be on secondary materials, remains a kind of tour de force.

Religion is "a cultural institution, a complex of symbols, articles of faith, and practices adhered to by a group of believers that are related to, and commonly invoke the aid of, superhuman powers and provide answers to questions of ultimate meaning" (Lewy, p. 4). Both books recognize that such a complex will normally serve to integrate personal lives and social systems. But, says Lewy, it may also be disruptive.

When people who want to effect social change do so in the light of religious visions, their movements will have double potency. Conversely, as Smith's authors recognize, when social change—"modernization"—comes to traditionally religious societies, its consequences can have doubly devastating effects.

Lewy summarizes his case studies: Millenarian revolts occur in situations of distress, when a group thinks about changing the world in the light of futurist promises, and often when a charismatic leader appears. Similarly, when colonized peoples come to consciousness, religion supplies a sense of national identity as against colonizing oppressors. Third, when leaders of established religious bodies find their spiritual or temporal goals to be congruent with a revolutionary force and lend it their support—watch out! Finally, individual thinkers in religious groups may provide potentially powerful ideology for social change.

These conclusions could be drawn on the basis of Western sources alone. Jewish messianism, the revolutionism of the left-wing German Reformation, the Puritan revolution's extremists—these are familiar exemplars of millenarianism. But Lewy also scans Hinduism, Confucianism, and Buddhism as well as the Biblical religions. That religion was a motor for change in China is clear from the instance of the Taiping Rebellion—ten to forty million dead in its aftermath—with its peculiar blend of religions old and new. The examples of the Taiping appropriation of some Biblical motifs and of post-Christian African messianism point to a curious irony: nineteenth-century Western Christian missionary expansion did not only help spread the religion that often integrated colonialist regimes. It also seeded the disruptive movements that later worked against these regimes.

Lewy seems most intent on pointing dispassionately to the complexities that are often overlooked by spiritual agitators. Only when he comes to turf most familiar to him—he has earlier written also on Catholicism in Spain and in Nazi Germany—does he seem to be involved. But he is never a simple partisan of Western religious "disruptiveness" for the sake of better futures. He points to the relative naiveté of today's Latin American Catholic theologians of revolution and their romantic North American armchair apologists. He quotes the writings of Jacques Ellul, who scolds Western academic revolutionary thinkers for searching for overseas causes that advance their own, rather than those of the people they profess to be helping. Overall, Lewy seems to be agreeing

with Robert E. Quirk in reference to one locale: "After all, in this imperfect world, perhaps the best solution to all human conflicts is a *modus vivendi*."

The comparative method seems to work rather well for Lewy. Revolution is made up of specifiable events, so he can often provide narratives before he extrapolates into system and patterning. Historians will feel more at home with him than with many of Smith's authors. They, after all, are discussing two elusive processes, each made up of millions of discrete and hard-to-define events. People can see, we assume, that "secularization" and "modernization" occur and can generalize about their effects. But what exactly is a secularized or modernized society? What do we make of the durability of religiosity in apparently postreligious societies? One deficiency in both books is the general absence of the people—the common people, the subjects and victims of revolution and modernization. We meet a few leaders. The habits, customs, manners, attitudes, and behavior of their followers are rarely dissected. We need to know more about how and why so many of them remain religious. (African Christianity, for example, is growing at twice the rate of the population growth, from three per cent of the population in 1900 to twenty-eight per cent in 1970.) What does it mean that there are now ninety-seven million Christians and many post-Christian syncretists along with Muslims by the millions—on a secularizing, modernizing, revolutionary continent?

Smith's previous work, referred to by contributors, and his introductory chapter give helpful structure to his symposium. His ambitious typology may be more attractive to sociologists and anthropologists or others who often think on nomothetic lines than they will be to serendipity-seeking historians. Models of five "polity-separation," "polity-expansion," "political culture," "political-process," and "polity-dominance styles of secularization" seem to be too stereotypical, yet somehow they work, and numerous contributors find them worth employing. At least one, Daniel Crecelius in a treatment of modern Egypt, also pointedly disagrees with Smith on them.

Leo E. Rose offers an illuminating portrayal of Nepal, a region long remote from modernizing tendencies; Norman L. Zucker discusses Israel as a secular society with a religious buttress; Smith himself joins Lewy and two of his own contributors—Harry Kantor and Thomas G. Sanders—in showing that Latin America never was and is not now as firmly or vitally Roman Catholic as many had regarded it to be

and that both Christian Democratic parties and Catholic revolutionary leaders know this. In another place he also is relatively hard on Gandhi, as was Lewy. Lewy's chapter on Nasirism and Islam, "a revolution in search of ideology," appears in slightly different form in both books.

Despite the hop-skip-and-jump character of Smith's samplings and the broad range of Lewy, readers will find both of these exercises in deprovincialization excellent reference volumes for their own comparative work. Most of them will in the future be likely to be more cautious than many have been to see revolution and modernization on the lines of Western cultural patterns as being normative. They may become less ready than traditional Western academicians have often been to see religious power simply to be dissipated or disappearing when it is merely changing. They may also be inspired to try to learn more about the people who are behind the patterns than these two enterprising books can begin to show them.

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FREDERIC COPLE JAHER, editor. *The Rich, the Well Born, and the Powerful: Elites and Upper Classes in History*. Urbana: University of Illinois Press. 1973. Pp. vi. 379. \$15.00.

The twelve essays in this volume deal, in a variety of ways, with eight different societies at various points over the past two thousand years. They are linked only by a general concern with elites, aristocracies, and upper classes. Like many such loose collections, this volume is something less than the sum of its parts, in both good and somewhat unfortunate senses.

The looseness is central to editor Frederic Cople Jaher's intention. Rather than forcing the essays into rigid comparative models, Jaher has preferred to let his authors adopt their own monographic focus, speaking to general concerns in a more suggestive and implicit voice. In addition to temporal and geographic variety, the collection exhibits a wide range of analytic approaches and studies various kinds of elites, from local cliques to societal ruling classes. The result is such a broad sampler that, as Jaher says, the essential test must be the quality of the individual essays.

By this measure, the results are quite impressive. Most of the essays are excellent, some exceptionally so. The authors manage to combine detailed focus with an ability to speak to the general interests of readers who cannot be expected to be deeply familiar with more

than a few of the periods, societies, or problems studied. The intellectual energy within the pieces is consistently high, and the resonance between them sustained. My own favorites included Paul Drake's sadly relevant analysis of Chile's tenacious upper classes; Richard Trexler's exploration of charity and poverty among the nobility of Renaissance Italy, an essay of surprisingly wide conceptual applicability; and a study of transitional Russia by David Ransel that makes effective use of an eighteenth-century letter-writing style guide as a source for social values and attitudes. Only one essay seems substandard, and it is unfortunately one of the more sophisticated methodologically: Thomas A. Krueger and William Glidden present a collective biography of the "New Deal Intellectual Elite" in the United States that is partly a circular demonstration of the obvious—concluding, for example, that intellectuals are better educated than other Americans, and that in a group of intellectuals there are proportionally more professors than in the general population "by a factor of about 32,000"—and partly an unsubstantiated streak of generalization through a room full of historical and theoretical straw men. But all the other essays are more grounded and substantial, and the collection as a whole, fluid and readable, nicely combines modern social science with the synthetic power of more traditional historical modes.

If the volume's looseness is largely intentional and effective, it also has its limitations, some of which must be attributed to a certain haphazardness. Better editing might have lessened the unevenness in the essays—Walter Arnstein's fine article on the Victorians, for example, is almost four times longer than the shortest essay, a disparity hardly required by substance. And there are other lapses, such as the unexplained omission of any information on the contributors, about whom, given the quality of the essays, many readers will be curious.

But there is a deeper problem involving the selections themselves. Although Jaher has been careful to include various modes and models, he has devoted little attention to securing a similar scope in the cultures selected for study. The resulting list—classical Greece and Rome, the Italian city-states, Transylvania, Chile, England (two), Russia (two), and the United States (three)—seems at first somewhat random, with obvious imbalances and European omissions. On closer inspection, however, this diversity reveals a narrowness that has unfortunate implications.

All of the essays deal with Western civiliza-

tion, and most, it turns out, share with Jaher's introduction appropriately Western assumptions about elitism, seeing it, for example, as a social fact in fundamental conflict with social and political values. But in other, non-Western societies, thousands of years of history have been built on quite different assumptions and definitions, as Chairman Mao's battle with the ghost of Confucius reminds us. Since Jaher has chosen to present a sampler, it is hard to see why he did not include as much cultural diversity as he does methodological. If comparative history is worth doing, it should be truly comparative, especially when the topic is such an essentially cultural expression as hierarchy, and especially in a collection that bears such a universal, unqualified title.

Whatever these limitations, the quality of the essays remains a tribute to authors and editor. Nobody will find every article of equal use, but almost any reader will be instructed in a variety of particulars and provided with an enormous amount of material, factual and conceptual, for profitable thinking in general terms about elites, culture, and history.

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Buffalo

WILCOMB E. WASHBURN, editor. *Proceedings of the Vinland Map Conference*. (Studies in the History of Discoveries.) [Chicago:] University of Chicago Press, for the Newberry Library. 1971. Pp. xvii, 185. \$10.00.

In January 1974 Yale University announced that its Vinland map was a forgery, thereby cooling one of the most hotly debated hoaxes of the century and dimming the luster of many learned opinions and theories expressed at the Vinland Map Conference convened by the Smithsonian Institution in 1966. It was at this symposium that foremost scholars had gathered to exchange views on "The Vinland Map and the Tartar Relation," published the previous year, and to account for much of what had transpired since the purchase of the map in 1957. The denouement did not evolve from the disciplines of cartography, philology, paleography, and history but from new techniques for analyzing the ink used to make the map, developed by McCrone Associates, who claim it could not have been manufactured before 1920. Tests of the Speculum and the Tartar Relation—two independent documents bound with the map—confirmed them as circa 1440.

Despite the diversity of views advanced and the intensity of convictions expressed, the two-

day conference produced some positive accomplishments. Besides providing an opportunity for many participants to inspect the map for the first time, it spurred a more determined analysis of wormholes, paper, leather, and ink. It also stimulated closer examinations of fifteenth-century maps. Linguistic disputes were partially settled, and a clearer picture of Norse medieval activities came into focus. The greatest differences seemed to be between historians and cartographers. The only allusion to the identity of the forger emerged when it was noted that the Latin phrases of Bishop Eirik's title printed on the map were the identical phrases used in a statement by Luca Jelic in 1891.

Although the integrity of the map was eroded by want of provenance—the source of the acquisition was never disclosed—most of the active participants at the conference believed in its authenticity. Proponents gave little credence to its lack of similarity to others of its era, despite the barebones appearance, misspellings, and careless translations and craftsmanship. It was said a professional forger would not have been so slipshod. Saga evidence remained intact, yet considerable recognition was attributed to the Eskimo and Norseman for their observations and cartographic abilities in presenting Greenland as an island. These assumptions were not supported by hard evidence, however.

Such discussions might have been influenced by the attendance of more outspoken doubters from Scandinavian and American universities, including Yale, who definitely suspected fakery. Many in England, Italy, and Spain cried fraud. Samuel E. Morison asked, "What do the northern islands illustrate other than some clever forger's appetite for dollars?"

Good hoaxes never die but are kept alive by sincere advocates who insist their contentions are still valid, hoping something will turn up to change the accuracy of scientific data.

A number of questions are still unanswered, such as: What is the origin of the map? Who was the forger? Who combined the map with the Speculum and the Tartar Relation? Who removed the Speculum from the Tartar Relation? Who are the unnamed people involved in presenting a baffling puzzle to the scholarly world? Weaving the answers into the third book of a trilogy entitled "The Truth about the Vinland Map" could well produce a best-selling whodunit.

JAMES F. NIELDS

Ware, Massachusetts

H. HOETINK. *Slavery and Race Relations in the Americas: Comparative Notes on Their Nature*

and Nexus. (Crosscurrents in Latin America.) New York: Harper and Row. 1973. Pp. viii, 232. Cloth \$13.50, paper \$3.95.

H. Hoetink is a brilliant Dutch sociologist who rejects most of the conventional wisdom about comparative slavery and race relations. As he first revealed in an earlier book, *Caribbean Race Relations* (1971), Hoetink regards his own work as a radical departure from the optimistic evolutionism that has dominated North American sociological (and historical) thinking about race questions for many years. In this volume he directly challenges previously accepted notions about racial developments in different parts of the Americas. Hoetink believes, first of all, that there was no causal connection between the character or even the existence of black slavery and the patterns of race relations that took hold in particular societies. He chooses to look at slavery in narrowly economic terms and finds it no different in origins and implications from other forms of involuntary labor, including the apprenticeship and contract labor arrangements that developed in most plantation colonies after emancipation. The nature of "race relations" was determined not by the labor system but by the larger social structure that existed outside of slavery and survived its abolition. From the beginning that social structure was essentially a racial hierarchy with the whites on top. But the composition and rigidity of the hierarchy were not the same in all areas. Hoetink distinguishes first between the two-category, white-black dichotomy in the United States and the threefold, white-colored-black division in all the other Afro-American societies. Then he makes a further distinction between the Iberian variant, where there has been considerable intermarriage between whites and light coloreds, and the West European variant, where the whites have been essentially endogamous. To explain the peculiarity of the North American arrangement Hoetink relies heavily on Marvin Harris's thesis that it was the presence of a uniquely large and influential poor white element that made the United States different in racial stratification from other New World societies. To explain why the West Europeans in general did not follow the Iberian example of intermarrying with light coloreds and thereby establishing an unbroken racial continuum between white and black, Hoetink restates his theory, more fully developed in *Caribbean Race Relations*, of the social role of "somatic norm images." The theory postulates that the dominant group in a multiracial "segmented" society has a more

or less fixed ideal of physical beauty that ultimately determines what somatic type will be considered eligible for full social acceptance, that is, intermarriage. Since the Iberian norm is darker than the West European, the Spanish and Portuguese had less revulsion to marriage with light mulattoes than the English, Dutch, or French, and the resulting racial hierarchy was therefore more continuous and less castelike in the Iberian societies than in the West European.

The implications of Hoetink's theories for the future of race relations in the West European societies, and especially the United States, are pessimistic in the extreme. While the Iberian societies seem to be headed in the long run for homogeneity through intermarriage, the West European societies can apparently become egalitarian only through the physical elimination of one or more of the endogamous segments. In the West Indies this would presumably mean the ejection of the whites (and possibly the white-oriented coloreds as well) by the black majority. One shudders to think what it might mean in the United States. Because his theory denies the possibility of equality in multiracial societies, Hoetink has been accused of endorsing a form of racism. He is certainly not a racist in the sense that he posits any inherent biological inequalities between groups of differing pigmentation, but if one expands the definition of racism to include the belief that a social order based on prejudice and discrimination will inevitably exist whenever there are perceptible physical differences between major groups, then the term is appropriate, as Hoetink freely admits.

The space available does not permit a detailed criticism of Hoetink's arguments. I find his demographic explanation for the North American two-caste pattern quite plausible, although it requires further documentation and historical investigation to be made fully convincing. But I suspect that his somatic-determinist explanation for the differences in the Iberian and West European variants can be undermined by the same method that Hoetink characteristically employs in his own refutations of other single-factor explanations—by finding exceptions. Detailed historical studies and more precise comparative consideration of particular multiracial societies, including some interesting cases outside the Americas, are likely to show variations—both over time and from place to place—that cannot be accommodated by Hoetink's law.

GEORGE M. FREDRICKSON
Northwestern University

JOHN ELLIS. *Armies in Revolution*. New York: Oxford University Press. 1974. Pp. 278. \$10.95.

Mr. Ellis's original book consists of seven studies of armies in revolutionary wars, but it has little to say about their operations or about orders of battle, tactics, or logistics. The author's interest lies principally in determining the relative importance of ideological and organizational factors in the cases he treats. It is generally true, he writes (p. 238), that "if the exercise of revolutionary power demands that some sort of armed force be created, then that army will only fight with any enthusiasm if military service is . . . seen to be an indispensable method of satisfying rank-and-file grievances." But such zeal is not in itself enough. "The exercise of power also demands effective administrative structures and strong central leadership, particularly in a time of bitter social upheaval." Indeed, these have often been sufficient to assure the success of armies that have failed "to harness the basic aspirations of the ordinary soldier" (p. 250), although this was often won at the expense of the original revolutionary ideal.

In Ellis's view, the Puritan army of the English Civil War and the revolutionary army of Mao Tse-tung have been the forces that most clearly embodied popular hopes and ambitions, although in the former case the unity and drive that was supplied by millenarianism broke down when it became necessary to make concrete decisions about the future shape of society. In the early days of the American War of Independence and the French and Russian revolutions, there was some integration between the basic preoccupations of the masses and the military modes adopted by the revolutionaries, but in each instance differences of view between the rank and file and the leadership forced the latter increasingly to use the discipline and hierarchical structure of the military system to control the dangerous tendencies of grass-roots sentiment. In the case of Prussia after 1806 we have an interesting example of a deliberate attempt to create a political ideology around which to build a revived military structure; the reformers succeeded in correcting the inadequacies of the military establishment but, since they had no real program of political and social reform, were unable to inspire a mass rising against Napoleon. In the case of the Paris Commune, on the other hand, the ideological preconditions for the creation of a genuine people's army existed but were vitiated by the absence of an effective and united revolutionary leadership.

Mr. Ellis's sympathies are clearly with the Chinese Revolution, which, he insists (p. 237), has made manifest "the potential of genuine people's war, the full appreciation of the integral links . . . between popular aspirations and military effectiveness" and, by doing so, has given "the established world order" a blow "from which it can never fully recover" (although he does not tell us what he means by this dark statement). Compared with it, most of the other cases discussed in his book (including the Russian one and by implication the American) are, in his mind, either examples of idealistic radicalism gone sour or of political zeal giving away to militarism and the cult of the state.

GORDON A. CRAIG
Stanford University

F. ABDULLAEV. *Iz istorii rusko-iranskikh otnoshenii i angliskoi politiki v Irane v nachale XIX v.* [From the History of Russian-Iranian Relations and English Policy in Iran at the Beginning of the 19th Century]. (Akademiia Nauk Uzbekskoi SSR, Institut Vostokovedeniia im. Abu Reikhana Beruni.) Tashkent: Izdatel'stvo "Fan." 1971. Pp. 133.

The stated purpose of this brief study is to prove that early in the nineteenth century, Britain, and to a lesser extent France, induced Iran to break her traditional friendship with Russia and assume an aggressive attitude toward her peaceful northern neighbor. As a result of foreign intrigue and domestic pressure exercised by a rapacious feudal nobility, Āqā Muḥammad Khān Qājār and his successor, Fath 'Alī Shāh, repeatedly attacked the Caucasus, whose peoples had eagerly sought and found Russian protection. The cruel, cowardly, disorderly, inept Persians were resoundingly defeated by courageous, virile, compassionate, humanitarian Russians, and Russia has continued ever after to be a friend of Iran.

Even though the author had access to Russian archives, he found nothing to modify this image of Russian policy, an image that would have embarrassed even the most imperialist Russian historians of the Tsarist period. To prove his thesis, Abdullaev plays with facts, withholds evidence, passes in silence over whole stretches of history, and produces a work the refutation of which would take hundreds of pages. Fortunately, this Russian nationalistic outburst by a man with a Muslim name in a book published in Uzbekistan is so obviously a political pamphlet that no refutation is necessary. One might simply comment in sadness that Abdullaev has

produced a book worthy of the darkest period of Stalinist historiography.

FIRUZ KAZEMZADEH
Yale University

D. K. FIELDHOUSE. *Economics and Empire 1830-1914.* (World Economic History.) Ithaca: Cornell University Press. 1973. Pp. 527. \$16.50.

In this long polemic the author elaborates, with some relatively minor adjustments, the major theme of his published research over the last fifteen years. He attacks a theory of economic imperialism he attributes to Hobson and even more to Lenin. Fieldhouse argues that the chief fault in Lenin's view—or "dogma" as he refers to it—lies in assigning to economic factors much too much importance in explaining imperialism generally and the colonialist scramble after the 1870s in particular. As against "economic factors," he prefers "political and social conditions" or "the power, prestige or security of the nation state rather than the wealth of its citizens" as the prime causes of imperialism. The volume's innovation lies in criticizing all theories that emphasize the European causes of European imperialism as "Eurocentric." Hence, his thesis is that imperialism before 1914 and especially after 1880 was not only more political than economic, but also more the result of changes at the periphery than of any changes in the European societies.

The book ranges widely over imperialist activities in nearly every region of Africa, Asia, and the Pacific. These regional case studies comprise nearly seventy-five per cent of the book, are based exclusively on relatively few secondary sources, and are presented so as to prove the thesis.

Fieldhouse's argument in this book, as in his other writings, suffers from two crucial, inter-related confusions. First, he posits a distinction—briefly and without discussion—between economics and politics that is extremely ambiguous. Certainly, neither Lenin nor any other serious Marxist thinkers accept or use the distinction Fieldhouse draws. It is, in short, a caricature easy to attack, and Fieldhouse accomplishes this with evident gusto.

Second, Fieldhouse mistakes the caricature he has drawn for the reality. The Leninist analysis of imperialism, contrary to Fieldhouse's assertion (p. 46), has undergone many significant clarifications and corrections within the Marxist tradition of social science over the last sixty years. Its basic analytical element is class. Imperialism is a stage in the evolution of a system in which the capitalist class predominates.

The expression of that predominance is at once economic, political, social, and cultural. The taking of colonies, itself but one stage of imperialism, emerges as the solution to problems encountered by the capitalist class in maintaining capitalism at a certain historical moment. Such problems, in this view, are at base economic but display of necessity political and cultural dimensions that may at any time dominate the consciousness of individuals or groups grappling with those problems.

Fieldhouse seems completely unaware of the complexity and subtlety of this view, and hence his work continually juxtaposes economic and political "theories" without adding anything to our knowledge of the obviously important subject of imperialism.

Finally, the argument that events at the periphery caused colonial adventures more than domestic developments in European societies is neither demonstrated nor clearly articulated. Of course, precise circumstances of time and place conditioned the differing specifics of colonialism in different territories. Yet what was unique about the nineteenth century in Africa and Asia was not the local events that occurred, but the fact that these provided the opportunity for colonialism, which returns us to the linkages between European capitalism and European colonialism. Moreover by 1830 a world market was already enough of a reality to influence many of the events at the periphery to which Fieldhouse accords an unjustified autonomy. Thus, to cite just one example, events in East Africa during the 1870s and 1880s are localized without linking them to the nineteenth-century slave trade there, which was the all-pervasive, externally caused economic reality of that region.

Since it is the basic argument and not the regional case studies that distinguish the point of this book, it is that argument which attracts critical review. If a careful, sophisticated critique of the Leninist interpretation can be made, it is, unfortunately, not made in this book.

RICHARD D. WOLFF

*University of Massachusetts,
Amherst*

ANCIENT

R. RIMANTIENE. *Pirmieji Lietuvos gyventojai (XI–IV tūkstantmetis pr. m. e.)* [The Earliest Inhabitants of Lithuania (11th to 4th Millennia B.C.)]. Vilna: Mintis. 1972. Pp. 95.

This is a comprehensive and enlightening introduction to the late glacial and early post-glacial history in the East Baltic area with the

main focus on Lithuania. The illustrations are largely from the author's collections from excavations in central and southern Lithuania.

The retreat of the ice sheet from the Baltic area and the onset of postglacial conditions—the warming of climate and the gradual spread of forests—exerted a profound influence on the cultural history. During the periods of climatic amelioration in the late glacial era at ca. 11,000 B.C. and ca. 10,000 B.C., known as Bolling and Allerød, the East Baltic area was reached by Upper Paleolithic hunters of several traditions: the Swiderians who moved upward from the Vistula basin in present-day Poland and the Magdalenians from northwestern Europe—the latter show associations with the Hamburgian, the Bromme-Lyngby, and the Ahrensburg complexes. The two ethnic components apparently mixed as is reflected in the character of flint complexes continuing Swiderian and Magdalenian traditions throughout the end of the late glacial period.

After 8000 B.C. the flint and bone tool kit reflects the adaptation to forest conditions—to woodwork, small-game hunting, and extensive fishing. Large scrapers and axes were used for various occupations with wood, bone harpoons and points for fishing and microlithic trapezes and leaf-shaped arrowheads for hunting. Analyses of charcoal from open hearths show the usage of pine bast for fire—there is no indication of felling large trees with flint axes prior to the Atlantic period. The cultural inventory of the Mesolithic during the Boreal climatic period in Lithuania has much in common to that of the Maglemose culture in Denmark. The author speaks, therefore, of another western wave of colonists in the East Baltic. The earlier hunters must have followed the retreating reindeer to the north. After this shift, the population of the East Baltic region remained static; the cultural continuum is very well evidenced by recent excavations of the peat bog sites dating from Atlantic times, the fifth and fourth millennia.

This subneolithic fishermen culture persisted until the infiltration of Indo-European speakers and ancestors of the Balts during the third millennium B.C., who were the carriers of the pastoral and food-producing Corded Battle-axe complex.

The book is in the Lithuanian language; fifteen thousand copies were published—and may now be sold out as is the usual case with scholarly books printed in the Baltic States.

MARIJA GIMBUTAS

*University of California,
Los Angeles*

WILSON E. STRAND. *Voices of Stone: The History of Ancient Cyprus*. Nicosia: Zavallis Press. 1974. Pp. 166.

In the introduction to this delightful little book on ancient Cyprus, Mr. Strand writes that "the road to any sort of history of Cyprus is unclear and poorly marked, but the journey is interesting and urgent." Fulfilling a genuine need for the generalist and interested layman, the author provides an up-to-date frame for the story, rather than history, of Cypriote man from the Neolithic Age to Hellenistic times.

In fewer than one hundred pages—about one-third of the text is taken up with an excellent selection of photographs, some never published before, and three maps—Strand argues a strong case for Cyprus as a single cultural unit with characteristically local traits in spite of strong influences from neighboring Mediterranean areas. By necessity, he relies on selected archeological evidence; yet in the conclusions he is careful to separate assumptions from facts. In ten chapters he treats the speculative elements in Cypriote beginnings, Minoan-Mycenean and Phoenician domination, and the unified struggle of Cyprus—notwithstanding mutually hostile local kingdoms—against Assyrian and Persian encroachment. A useful index and a chronological chart conclude the book. It is to be regretted that a bibliography is missing and that references are occasionally limited and unclear.

The book does not solve problems for specialists. Rather, it motivates the reader to approach the entire question of Mediterranean interactions with a fresh point of view. The author expresses the hope that this endeavor could be a first step toward "a larger and more comprehensive history of an island whose historical significance can no longer be ignored." The book admirably fulfills these expectations.

G. J. SZEMLER
Loyola University,
Chicago

MICHAEL VENTRIS and JOHN CHADWICK. *Documents in Mycenaean Greek*. Second edition by JOHN CHADWICK. With a foreword by the late ALAN. J. B. WACE. New York: Cambridge University Press. 1973. Pp. xxxiv, 622. \$37.50.

The famous book *Documents in Mycenaean Greek* (1956) by Michael Ventris and John Chadwick is probably the most original and influential work in classical scholarship since World War II. Like any classic it needs to be kept alive in print, intact; it should also

be allowed to reflect the advances in the discipline it helped to found. Chadwick has designed a solution that resembles the scholia on an epic, the old text kept in its old "pure" form, with a large additional commentary correcting it and reinterpreting many lines and ideas.

Documents was not revolutionary; the Linear B revolution had already been sparked by Ventris's and Chadwick's "Evidence for Greek Dialect in the Mycenaean Archives," in the *Journal of Hellenic Studies*, 73 (1953), by E. L. Bennett's *The Pylos Tablets* (1955), and by university lectures. When *Documents* first appeared there was a great volume of work on Linear B by other scholars, to which the authors could refer with meticulous pleasure. An international discipline developed in three years, offering such major contributions to problems in Greek language, history, archeology, economy, social structure, and religion that no single scholar can be expected, now, to maintain authority over all aspects of the field. In the past eighteen years the knowledge and the number of investigators have grown at a perplexing geometric rate, with four or five new journals devoted entirely to Linear B and related studies. Although the Zakro Linear A tablets have remained, shockingly, unpublished, and the Linear B texts from Thebes have been slow to reach the public, the plea that marked most reviews of the original *Documents*, for more systematic investigation of contexts, categories, and linguistics, seems now largely answered. That this expanded *Documents* should appear at about the same time as Bennett's new *Pylos Tablets in Transcription* seems a sign of the end of the first generation of Linear B studies and an inauguration of the second.

Ventris's death in an automobile crash at the moment *Documents* first appeared was one of the more stupid, painful losses to classical scholarship. The premature extinction of the collaboration with Chadwick left the new discipline palpably diminished. Although colleagues in twenty countries lent their best energies to more accurate readings and more plausible interpretations of the Mycenaean texts, a remarkable power of imagination and synthesis had really disappeared. It is as though Sir John Beazley had died after the first *Attic Red-Figure Vase-Painters*. Subsequent and often brilliant work by Bennett, Chadwick, Chantaine, Heubeck, Killen, Lang, Lejeune, Morpurgo Davies, Mühlestein, Palmer, Risch, Ruijgh, and Ruipérez, with many others, has made the state of the discipline far more persuasive and precise than

when *Documents* first became general reading, but a coherence and energy seems lacking.

In this context Chadwick's decision to reprint *Documents* unchanged was courageous and difficult to organize. All the rash enthusiasms and inspired but mistaken guesses are pristine, with the margins freely daggered to lead to the 140 pages of revisions and additional commentary. It is pleasant to see old friends like the thirty-seven female bath attendants at Pylos come through not even daggered, and for the six sons of the headband makers and musicians and sweepers the dagger leads only to a mild comment, "guesses which now appear improbable." The old numbers for Pylos texts were partly changed to new ones in the first edition, partly not; now the new section uses the new Knossos numbers while the old part keeps the old; the Concordance may be the most thumbed-over part of this book. New syllabic values like *qa* for *pa*, *85: *au*, or *91: *two* are naturally used throughout the new part, not the old. Chadwick has treated the problems, which are not few, in such an organized manner that the book can still be used as a unit although it is really two books. There are new texts and an excellent glossary.

The additional commentary is almost entirely linguistic, as one expects from a leading philologist. Its scope is more restricted than in the old *Documents*. It often shows the earlier light-hearted exploration for possibilities: in a text about a drug (*pharmakon*), "*a-wa-ra-ka-na* must be an *a*-stem with a genitive singular *-ao* if masculine, or plural *-āōn* if feminine. It is easier to make fun of the suggestion that it is a Mycenaean form for *arachne* [spider] than to suggest anything better; a derivative in *-nā* of *awlax* > *aulax* 'furrow' is theoretically possible, but unconvincing for sense" (p. 505). Alternatives are offered so freely that one must be trained in the field to decide. The archeology is also a bit light hearted; the Sea Peoples swarm off Pylos, and the bronze of the kingdom is made of melted votives from undiscovered shrines. These are not defects but part of the classic flavor, to which so much has now been added in vocabulary, grammar, and context analysis, in such a systematic, accurate, and informed style, beautifully printed and designed, that Chadwick should be immensely proud of his monumental contribution to the field and to the work of his dead colleague. The new *Documents* cannot, in the nature of things, hold the same commanding position its predecessor held, but to my knowledge it has no rival.

EMILY VERMEULE
Harvard University

ERICH S. GRUEN. *The Last Generation of the Roman Republic*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xiv, 596. \$18.50.

"The writing of history is forever plagued by the temptations of hindsight." In this spirit Professor Gruen approaches the thirty years from the abdication of Sulla the Dictator in 79 B.C. to the outbreak of civil war in 49 B.C. To mid-50 B.C. the Republic continued functioning normally; not only did no one intend or anticipate the catastrophe, but the war and the end of the Republic were avoidable. Hence the quotation above, and the almost certain controversy that the book is bound to provoke, for Gruen has challenged many of the ripest, most cherished conclusions of a long scholarly tradition.

Concentration is almost exclusively on internal affairs. The first three chapters are largely introductory. Two prosopographical chapters follow; Gruen argues that the electorate continued to choose men from noble families in the same proportions as they had for generations; hence the Senate, whose ruling clique was composed of these elected officials, also remained essentially unchanged. The next chapter treats criminal and administrative legislation; Gruen concludes that far from showing ineffectualness in coping with pressing problems, the Romans rather overdid it: the "proliferation of statutory law may have contributed to the Republic's undoing." Two chapters on criminal trials follow; Gruen argues that their political implications and effects were not significantly different from the period treated in his earlier book, *Roman Politics and the Criminal Courts, 149-78 B.C.* (1968). The next chapter concerns the behavior of the city plebs and the army, the penultimate instances of discontent and violence. In the former Gruen sees no evidence that the lower classes desired political or social revolution, in the latter no reason to think that instances of disruption were significantly different from what they had been in the past: violence in Rome had a long pedigree. The last chapter treats the outbreak of the civil war. The causes were immediate: the conservatives' attempts to split Pompey from Caesar and the actions of the renegade tribune C. Curio.

Books that challenge orthodoxy are healthy; they force re-examination and rethinking. Certainly too much of the scholarship has been bedeviled by post eventum interpretations: this may well not have been a "crisis without an alternative." On a great many matters the book is incisive and persuasive. But was it really

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"business as usual" in the late Republic? Take violence as an example. That there were precedents for it, that those who used it did not aim at revolution, and that stringent measures were passed against it are correct. Yet the problem is of degree, not kind. Violence occurred in this period with a frequency and intensity unknown before, and it was adopted by people who deplored it as they used it (for example, the virtuous Cato). The changes that led to the catastrophe were slow and gradual, but no less profound for all that. The historian Livy described the end in terms of a building that settles, deteriorates, and suddenly collapses. Up to almost the last moment one could point to the familiar features: foundation, supports, and roof—together with the people continuing to live inside it. Gruen makes his reaction to this interpretation clear: it should not be assumed to be true, but proved.

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ROBERT K. SHERK. *The Municipal Decrees of the Roman West*. (Arethusa Monographs, number 2.) Buffalo: Department of Classics, State University of New York. 1970. Pp. 111.

This monograph fills a gap in scholarly literature by collecting and analyzing systematically all known inscriptions recording municipal decrees in the Roman West. The purpose is laudable, granted the importance of such information for our understanding of city life, especially appreciation of the extent to which Romanization came to mean uniformity in the institutions and procedures of local government. The core of the work (pp. 17–58) consists of the texts of some sixty-five decrees, most from Italy, a few from Gaul and Africa, none from Britain and Spain. Not surprisingly, most of them are datable to the period between the late Republic and the middle of the third century A.D., when municipal life was most vigorous. Wherever possible the author also uses the few surviving literary sources on his subject to supplement the epigraphical information.

He concludes the following. By the late Republic municipal senates in general had absorbed the procedures of the Roman Senate, their decrees conforming in phraseology and organization to *senatus consulta*. By the same time it had become normal practice for municipal decrees to be registered in local archives. Most of them honored worthies for their community service; some dealt with the appointment of officials, others with miscellaneous matters pertaining to municipal life. The ap-

pointment of *curatores* beginning ca. A.D. 100 limited the independence of municipal senates, since *curatores* could interfere with the legal force of decrees. Such interference by imperial agents culminated during the late empire when towns were in decay. The author concludes by denying any continuity between Roman urban institutions and those of medieval cities arising in the eleventh century.

This workmanlike monograph serves its purpose, even though its conclusions do not seem to be startlingly new or unexpected. One can only hope that more inscriptions will be discovered to improve the very patchy nature of the evidence.

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MEDIEVAL

BERNARD S. BACHRACH. *Merovingian Military Organization, 481–751*. Minneapolis: University of Minnesota Press. 1972. Pp. viii, 157. \$7.50.

The thesis of this work appears to be that the Merovingian military system derived more from Roman military formations of the late fifth century than from German antecedents. This is a remarkable thesis since it is not sustained by the evidence discussed by its author. Indeed, the evidence cited indicates clearly that the single, crucial issue determining the military fortunes of the Merovingian kings was the support of the magnates.

It should be pointed out that Bachrach's argument that "Romania" outweighed "Germania" recalls the racial-nationalist controversies of the nineteenth century. Moreover he never analyzes any evidence in support of the thesis. His references to such evidence are casual in the extreme—possibly because it does not bear close scrutiny. Finally, the superficial nature of the case for "Romania," as stated by Bachrach, is clearly indicated in that nowhere in his discussion is "Romania" defined. Obviously, given the economic contraction of the Roman West under the late empire, we cannot assume continuity of function, structure, training, and composition between the so-called Roman formations of the fifth and sixth centuries and the earlier Roman regiments or other regimented formations. Considering the economic context of the fourth and fifth centuries, we must suppose a nearly common level of material culture to have obtained for both Romans and Germans, and we should indeed ask whether the

supposed Roman/German dichotomy is of any use to us.

DAVID HARRY MILLER
University of Oklahoma

HERBERT JANKUHN *et al.*, editors. *Vor- und Frühformen der europäischen Stadt im Mittelalter: Bericht über ein Symposium in Reinhausen bei Göttingen vom 18. bis 24. April 1972*. Volume 1. (Abhandlungen der Akademie der Wissenschaften in Göttingen, Philologisch-historische Klasse. Third Series, number 83.) Göttingen: Vandenhoeck & Ruprecht. 1973. Pp. 337, 32 plates. DM 88.

Following suggestions by the Committee on Early Urbanism, founded by the Union Internationale des Sciences Préhistoriques et Proto-historiques, archeologists and historians, who get their information mainly from written sources, assembled near Göttingen in April 1972 to exchange views on the formative period of urban settlements in Europe north of the Alps. This volume presents the papers that were read on terminology, Roman-medieval continuity, and the early developments in the middle and the northwestern parts of Europe; those on northern Europe, eastern Central Europe and southeast Europe are to follow in due course, as Professor Jankuhn points out in his instructive introduction (p. 11).

Viewpoints of the legal historian, the geographer, and the sociologist on the notion of urban settlement in the early Middle Ages are represented by Gerhard Dilcher, Dietrich Denecke, and Horst Callies (on Max Weber). Gerhard Köbler tries to fix *wik* to the sense of *vicus*, *dorf* (pp. 61–76). More convincingly Herbert Ludat renews his explanation of West Slavonic *město* for “town” since the thirteenth century as a reflex of the earlier split of meaning in German *stat* and therefore as a consequence of German settlement in the east during this time (pp. 77–91). The structure of later Roman towns as administrative, religious, and cultural centers of their *civitates* is the theme of Friedrich Vittinghoff (without references), while the transformation of those communities into medieval settlements is discussed by Hans Schönberger for the Rhine and Danube areas (pp. 102–09), Otto Doppelfeld and Reinhard Schindler for Cologne and Trier respectively (pp. 110–29; pp. 130–51), and Martin Biddle for Winchester (pp. 229–61). P. V. Addyman on Saxon Southampton (pp. 218–28) may point to some sort of continuity, too, because Roman Clausentum nearby was defensible even in the time of Alfred the Great. Even

for Reccopolis, on the town hill sixty kilometers east of Madrid, Klaus Raddatz thinks of later Roman roots (pp. 152–62); but W. A. Van Es does not believe in continuous settlements at Dorestad from Roman imperial times onward (pp. 202–17), and Norbert Wand presents—though finds since the late Paleolithic period are known—the Büraburg near Fritzlar as a fortified town from the later seventh to the middle of the ninth centuries (pp. 163–201). The market as an early form of urban settlement in Germany is the copiously documented and lucidly presented thesis of Walter Schlesinger (pp. 262–93); Ernst Nickel on Magdeburg (pp. 294–331) and Paul Grimm relating Ottonian markets to fortifications in the Elbe-Saale region (pp. 332–37) give notice of recent excavations.

The most impressive paper besides Schlesinger's is that of Biddle, who pleads for Wessex kingship as an element of continuity in a town without finds from 450 to 650 and for a complementary idea of “town”—leaving out the individual markets on his table of supplementary urban elements in Winchester and Southampton (p. 247). But the most sensational finds are those of Ernst Nickel. He has unearthed, north of St. Mauricius at Magdeburg, the foundations of the western part of the palace of Otto the Great, on a symmetric plan with two inner and corresponding outer crypts and with pilaster strips, reminding one of Byzantine buildings and not of the plain and rectangular *palatia* otherwise known from Germany.

KURT-ULRICH JÄSCHKE
University of Marburg/Lahn

HEINZ LÖWE. *Von Cassiodor zu Dante: Ausgewählte Aufsätze zur Geschichtschreibung und politischen Ideenwelt des Mittelalters*. Berlin: Walter de Gruyter. 1973. Pp. 342. DM 112.

Professor Löwe of the University of Tübingen offers in his volume, *From Cassiodorus to Dante*, a selection of twelve essays that, for the most part, were originally delivered as lectures between 1948 and 1970. The connecting link between the essays is their general field, which is medieval historiography; furthermore the author's methodical approach to explore, through the works of the medieval authors, the leading political and social ideas, or—as Löwe expresses it—the “world of political ideas” (*politische Ideenwelt*) of the Middle Ages. His aim is to convey to the reader the true spirit and mentality of an era. While dealing with medieval historiography he is contributing effectively

to the social and cultural history of the Middle Ages, and his essays could thematically be classified as studies in the history of ideas (*Geistesgeschichte*).

The first six essays concentrate on the problem of transformation of the late antique Roman world and the emergence of a new society in the West, dominated by a semibarbarian Germanic nobility, exposed to classical reminiscences, and increasingly imbued with the new Christian world view. The volume starts with an analysis of the personality and works of Cassiodorus, whose life and activities authentically mirror the merger of two cultures. He, as a Roman and a devout Christian, at the same time was an interpreter of the policy of peaceful co-existence between Catholic Romans and Arian Goths, under the tolerant regime of the Ostrogothic king Theodoric.

With the shrinking of the Eastern emperor's authority in the West, there appears a new concept of universal authority, supplementing the missing authority of the Roman emperors; this is the *Imperium corpus Christi*, the spiritual empire of Christ, which eventually will manifest itself in the Church of Rome and will dominate the civilization of Western Europe throughout the Middle Ages. The evolution of this new authority and the complete disengagement of the Germanic West from the Eastern Roman Empire is the general theme of the essay "From Theodoric to Charlemagne."

In the second group of essays Löwe examines the institution of the medieval German-Roman Empire and the scope and limits of the imperial authority in the interpretation of the historiography of the Carolingian era, as well as of the Ottonian and early Salian period. He comes to the conclusion that the only thing that distinguished the German emperors from the rest of the European monarchs, and gave them the aura of universal authority, was that they controlled large areas outside Germany, in Italy and Burgundy; furthermore, that since the time of Otto the Great they usually assumed the role of defenders of the papacy and the Church, which was, in fact, the only universally recognized supreme authority in the Christian West.

In the last two essays, "Dante and the Stauffers" and "Dante and the Emperorship," we are told that Dante had little sympathy for the German emperors. The only Hohenstaufens who received favorable appreciation in the *Convivio* and the *Divine Comedy* are Frederick II and his illegitimate son Manfred, whose mother was an Italian noblewoman. Ap-

parently Dante's Ghibelline, proimperial political stand can be attributed less to his Germanophil sentiments than to the realistic appraisal of the political situation in Italy at the beginning of the fourteenth century. In the person of the emperor Henry VII of Luxembourg, Dante welcomed not the German monarch but the cosmopolitan superpower which alone could bring hope for Italy and the whole of humanity.

It is regrettable that from the selection of medieval works and authors some well-known and fairly important names, like Liutprand of Cremona or Otto of Freising, are missing. As it is, the presentation of the medieval world of ideas—the *politische Ideenwelt* of the Middle Ages—remains somewhat inconclusive. On the other hand, the volume's great merit is that in it Löwe brings to the surface works and authors of the early Middle Ages—like Ardeo, bishop of Freising or Regino, abbot of the monastery of Prüm—whose names are less known to the general reader, but who nevertheless reflect with great intensity the spirit, the ideas, and the acting forces of their times.

ANDOR URBANSKY

University of Bridgeport

F. W. STERNFELD, editor. *Music from the Middle Ages to the Renaissance*. (Praeger History of Western Music, volume 1.) New York: Praeger Publishers. 1973. Pp. 524. \$20.00.

There are both advantages and disadvantages in books put together by a score of contributors, as is the case in this first volume of a series of five edited by Dr. F. W. Sternfeld. The synthesis of many talents makes for a diversified viewpoint, and the often different approach and style of each author adds a certain interest of its own. On the other hand, a multiplicity of methods can—and does in many instances—cause a sense of confusion unless rigidly controlled by a master mind; and since the history of music ranks as one of the most complex in its ramifications through time, language, and the frontiers of taste and topography, the clarity of its exposition depends upon careful organization and lucid prose.

Those authors who most nearly succeed in this task are few in number. Two outstanding French scholars, Nanie Bridgman and François Lesure, share between them the rich tapestry of French music from its *grand siècle* guided by Vitry and Machaut—both poets as well as composers—up to the late Renaissance, when instrumental works appeared in abundance and

the *Ballet Comique de la Reine* set a standard for all future courtly entertainments. John Caldwell writes with knowledge and understanding of the music of Josquin, Clemens non Papa, and Lassus, but the international career of the last mentioned must also be studied in at least three other chapters. Fortunately there are few composers who suffer in this way, and it is largely due to the editor's skill that the major figures tend to remain in one piece.

As a paragon to the French chapter, Federico Ghisi's account of Italian music from Landino to Monteverdi commands the highest respect. The all-important literary influences are shown in clear perspective, and the grandeur and misery of patronage both receive fair treatment. Even the nonspecialist reader is led persuasively through the thickets of bibliography and stylistic analysis, so that he can see from a better vantage point the cosmopolitan ingredients that went to perfect the motet and madrigal. There are, inevitably perhaps, a few debatable points; and where some have called the text of Monteverdi's *Tirsi e Clori* "marinistic" or attributed it to the younger Striggio, Ghisi assures us that the author was Guarini but offers no evidence to support his opinion, which nevertheless carries considerable weight.

Two notable contributions from American scholars are those of Joseph Kerman, whose account of English music after the Reformation is both learned and succinct; and Robert Stevenson, who writes authoritatively on music in South America. Much less impressive are the two chapters on medieval music in England by Ernest Sanders, who appears to misunderstand the functions—within a polyphonic context—of sequence, trope, prose, and responsory, besides giving evidence of a poor knowledge of Latin and of the liturgy. Although the greater part of his material is derived from the work of other scholars, their names are cited so infrequently in the footnotes that the reader cannot help but contrast this procedure with the conscientious and accurate documentation supplied by other writers. In chapter 3 the names and works of only five scholars appear in as many as sixty-six footnotes.

It is thus extremely difficult for the reader to sort out the confusion in the account of the Compostela music, and it is virtually impossible to do so in the brief discussion of motets in a Princeton manuscript. These were first brought to light by Kenneth Levy in 1951, yet his valuable article containing facsimiles and transcriptions is cited neither in the footnotes nor in the general bibliography. Later material, quite

clearly based on the researches of Bukofzer and Harrison, suffers a similar fate, which seriously reduces the usefulness of the volume.

DENIS STEVENS

Columbia University

SIDNEY R. PACKARD. *12th Century Europe: An Interpretive Essay*. Amherst: University of Massachusetts Press. 1973. Pp. ix, 362. \$12.50.

The twelfth century is for many historians the most fascinating of all the medieval centuries—and the most difficult to grasp whole. The sense of a new lease on life that permeates every aspect of human endeavor and the amazing diversity that existed throughout Europe make the twelfth century a compelling subject. Each new book on the period is interesting to read, if only to see how the author chooses to thread his way through the immense amount of primary and secondary material that is available. Packard has written a stimulating essay filled with his reflections on the way the century should be approached and his conclusions about the underlying factors that gave form and coherence to the period.

Packard begins by presenting his case for the twelfth century, which he dates from 1096 to 1215, as a watershed in European history. It is, as Packard emphasizes, an age linked to its past, but just as surely an age in which there were many developments that continued into the later centuries and are with us still: the growth of towns, the increased organization of the feudal system and of agriculture, the reorganization of the structure and doctrine of the Roman Church, the innovations in law and government, to name only a few. Packard emphasizes the point that "Europe" is not a broad enough term, because it is generally taken to mean Western Europe and that the proper area to study in Christendom includes Central and Eastern Europe. One of the real virtues of his book is that he includes material on Kievan Russia, Scandinavia, the Balkans, Byzantium, Islam—in fact, all of the civilizations and peoples with whom Western Europeans had contact. It is part of Packard's thesis that one of the major factors that contributed to the innovation in the twelfth century was the existence of borders open and accessible to a rich variety of cultural influences.

Another factor Packard is convinced must always be taken into account in order to explain the quantity and quality of change and the exuberant energy that characterize the century is the change in the population: the remarkable

growth in population, the mixture of peoples, and the predominant youthfulness of the society. He discusses in detail the statistics for each country and for many of the cities.

The material is organized topically, and Packard explores in turn the economy; the Church; the intellectual, literary, and artistic developments; and the "states" (the quotations are his) of Europe. Packard skillfully weaves together substantive information, recent scholarly research and opinion, and his own reflections on every aspect of the twelfth century—all in a style that is direct and unpretentious. It is a knowledgeable book, useful for scholars and students alike—clearly the work of a scholar who has thought deeply about his subject and is not reluctant to share his personal observations with the reader.

JILL N. CLASTER
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ROBERT E. LERNER. *The Heresy of the Free Spirit in the Later Middle Ages*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xv, 257. \$10.00.

This study, based on extensive archival research, carefully re-examines those scanty historical records concerning the Free Spirit heresy. For two centuries, beginning in the early thirteenth century, Church officials complained about certain heretics, many of them women, who called themselves Free Spirits. These heretics, so the complaints read, claimed to have progressed so far up the mystical ladder that they were one with God and beyond moral law. Their detractors said this freedom led to gross immorality.

Lerner insists that these complaints, frequently found in Inquisition records, should not be taken at face value. In his view the Free Spirits were neither as numerous nor as evil as claimed. Once having identified heretics of this type, inquisitors continued to "find" them. Lerner alleges that certain early denunciations of their teachings provided models for the suspiciously similar interrogations and reports that followed.

Lerner challenges many reports of illicit behavior. He notes that the accused were often associated with Beguines and Beghards, lay religious groups devoted to poverty and the common pursuit of the apostolic life. Perpetually in disfavor with Church officials, since they assumed without authorization the form of a religious order, these devotees of poverty were treated with contempt because they refused to

obey orders to disband and because they did not work. Churchmen and laymen alike had reason to dislike them and to believe readily accusations of immorality against them.

Lerner reports, however, that in some circles the Beguines and Beghards were lauded for their devout behavior. Further, claims of mystical advancement beyond the constraints of moral law did not necessarily produce immoral behavior. Quite the opposite, the avid pursuit of the apostolic life by some of these irregulars may have aroused resentment and unfair accusations. In sum, the study contends that often the records tell us more about the accusers than about the accused.

In order to sustain his contentions, Lerner is sometimes forced to engage in speculation. For example, referring to a compilation of heretical teachings written by Albertus Magnus, Lerner suggests that it might be a second-hand compilation, not firsthand evidence, "the sort of pigeon-holing that a learned man like Albert could have done in the comfort of his study" (p. 15). Again, the reader is asked to "wonder" if the apparently extreme positions advocated by the Free Spirits resulted from the "way the questions were posed." Or again, perhaps the extreme positions flaunted by the heretics were examples not to be taken literally (p. 18). Thus the author's interpretation is a hypothesis, well argued to be sure, but not a thesis beyond challenge.

If the author is correct, we know less about the Free Spirit heresy than was previously thought, and the movement should be assigned a less prominent place. The study quite properly debunks Norman Cohn's attempts to glorify these dissidents. The volume illustrates very clearly the difficulty of writing history from documents condemning heresy.

PAUL L. NYHUS
Bowdoin College

COLIN PLATT. *Medieval Southampton: The Port and Trading Community, A.D. 1000-1600*. Boston: Routledge and Kegan Paul. 1973. Pp. xvi, 309. \$23.00.

The fame and importance of medieval Southampton were not wholly dependent upon the town's antiquity or its size. It had been a minor Anglo-Saxon settlement until the century of the Conquest, and its maximum population—about two thousand—made it a prominent but hardly a major urban center. Rather, its unique site and its position on the trade routes with the Iberian Peninsula and the Mediterranean

brought it renown, wealth, and—as in the disastrous French raid of 1338—danger. It grew, it suffered, it grew again, and it fell again: this story neatly fits into the years between 1000 and 1600.

Southampton had also served medieval historians well. Its extant records are extensive and detailed. The Southampton Record Society has been editing and publishing primary materials since 1905, and the society's volumes, the traditional narrative history (by J. Silvester Davies in 1883), and scores of special studies, local notices, articles, and dissertations make it, after York and the university cities, about the best known of the provincial towns—or, after its full charter of liberties in 1445, boroughs. On this shelf of worthy items there is ample room for Platt's book.

Platt has not written a full monograph on the community. He offers us neither a detailed study of municipal government (as did Gwyn Williams, *Medieval London, from Commune to Capital* [1963]) nor an analysis of a ruling elite (as did Sylvia Thrupp, *The Merchant Class of Medieval London* [1948]) nor a rounded narrative of urban growth and its problems (as did Sir Francis Hill, *Medieval Lincoln* [1965]). Instead he presents a series of chapters about Southampton. They move forward chronologically and read like separate—and mostly excellent—essays on various aspects of municipal life, for example, "Topography of a Prospering Borough" (ch. 5) or "Trade, Distribution, and Supply" (ch. 13). It is no use asking for continuity or for neglected topics; what is omitted, or put into an appendix but not integrated, is just not there. But what is there is mostly first-rate. The tale of the two great peaks and troughs of prosperity is regularly brought home through the incisive quotation, the detail culled from a manuscript, or—as befits an archeologist, about to publish *The Excavation of Medieval Southampton*—the fruits of a dig. Municipal quarrels, the size of houses and their ground plots, the diets of the rich, the state of the walls and fortifications, malfeasance in office, the effects of the plague, the rebuilding in timber rather than in stone, all have their moment. When I say that Platt offers a kind of historical journalism, I speak in commendation of his efforts.

There are not only the usual plates, diagrams, and maps, but a long prosopographical appendix (with 102 biographies from 87 different Southampton families), data on town size and population, and some "tenement histories." For urban historians, economic historians, and all

with friends and relatives in Southampton, a recommended Christmas selection.

JOEL T. ROSENTHAL
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Stony Brook

JAMES A. BRUNDAGE. *Richard Lion Heart*. New York: Charles Scribner's Sons. 1974. Pp. 278. \$10.00.

Professor Brundage is one historian who dares to put into practice the admonition that the professional historian should write for the general reader and not abdicate that responsibility to the journalist or amateur historian. In this book he also decided to focus rather narrowly on the events of Richard's life on the grounds that Richard took little or no interest in the problems of government. Unfortunately this limitation rules out most of the gains made recently by historians in their understanding of the period through careful study of the records produced by an expanding government in England. It also means that the new biography rests primarily on the same narrative sources used by Kate Norgate in her 1924 biography of Richard. A reviewer must agree with Brundage that the older book is not superseded; readers who want a more detailed narrative and footnote references to those sources will still need to turn to the Norgate biography. Brundage, however, gives a more explicit interpretation of Richard than the somewhat romantic view that Norgate took over from the chroniclers, and his chapter on Richard's character is the most valuable one for historians. The general reader for whom the book is intended may not find it as exciting as Philip Henderson's *Richard Coeur de Lion* (1959), but he can be assured that the new biography is more reliable.

The least satisfactory part of the book deals with the early life. The relationship with King Henry II is obscured by the slight treatment of Richard's part in the revolt of the sons against the father in 1173–74. The summary of the next years when Richard was proving himself militarily and politically in Aquitaine is perfunctory, and the account of the siege of Taillebourg in 1179 fails to convey any sense of why contemporaries were so impressed by this victory won by the young Richard. The account of that incident suffers by comparison with that by Kate Norgate or the superior discussion of the whole early period in W. L. Warren's *Henry II* (1973). The best narrative chapters forming the core of the book emphasize

Richard's participation in the Third Crusade and reflect the author's own interests developed in his earlier books on aspects of the Crusades. In other books Richard's role as a poet may have been exaggerated, but the reader of this book would scarcely guess that Richard shared his family's interests as a patron of poets and troubadours. The treatment throughout sticks to a narrative of events and battles in a rather old-fashioned manner. To be sure, current scholarship—and Brundage in this book—has gone far in deromanticizing the Lion Heart, but something has been lost in the process when almost nothing remains that would explain why Richard became the subject of legend and romance. In spite of its unevenness, this biography should be recommended to anyone with a general interest in the subject, and the historian would do well to consider the author's interpretation of Richard's character. Within his self-imposed limits Brundage has demonstrated that the historian has something to offer the general reader.

CHARLES R. YOUNG
Duke University

GUY DEVAILLY. *Le Berry du X^e siècle au milieu du XIII^e: Étude politique, religieuse, sociale et économique.* (École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisations et sociétés, 19.) Paris: Mouton & Co. 1973. Pp. 636. 96 fr.

The state doctoral thesis of Guy Devailly, professor of medieval history at the University of Rennes in Brittany, is a history of the central French province of Berry (the modern departments of Cher and Indre), the capital of which is the city of Bourges. Devailly divides the three and one-half centuries of his history into three periods breaking at around 1080 and 1180. During that time the people of Berry (Berrichons) experienced most of the vicissitudes of their fellows in other French provinces: disappearance of royal power at the beginning, then rule by a local aristocracy, finally reassertion of royal authority at the end; decadence of the regional church, then intense religious reform; growth of population and rural expansion; and the beginning of the emancipation of the peasant population in the thirteenth century. Devailly's book falls into the category of the provincial monograph, a category distinguished in recent decades by works of French and Belgian historians like Georges Duby, Léopold Genicot, Marcel Garaud, Ga-

briel Fournier, and Philippe Dollinger. Like them Devailly bases his work mainly on the careful analysis of hundreds and thousands of charters, many unpublished. Like them he is concerned with the history of the aristocracy and peasantry and with economic development. But instead of limiting himself to social history, Devailly has sought to draw a fuller picture of life in medieval Berry. First of all he has given considerable emphasis to political narrative. Slightly more than a quarter of the book is devoted to describing how the houses of Blois-Champagne, Anjou, Aquitaine, and Plantagenet struggled to gain control of Berry until the Capetians finally succeeded in the thirteenth century. Then the history of the Church, and especially the Gregorian reform movement, commands almost as much attention as political history.

To write a monograph of this kind requires immense reading, since the conscientious author has to familiarize himself with the latest research in a host of different subjects ranging from religious reform to drainage of swamps and growth of towns. From my vantage point Devailly has done this task well, but I think he would have enhanced the already considerable value of his work by more diligently comparing his findings with those of other historians on the same subjects. Rarely does he refer to social historians other than Duby and Genicot, and the relevant works of a number of contemporary scholars such as Robert Fossier and Fournier are not even cited in the bibliography.

Despite the space given to political and ecclesiastical history, the bulk of the book deals with questions of society and economy. Because of deficiencies in the sources, the sections on the aristocracy lack the subtlety, the depth, and the richness of detail to be found in Duby and Genicot. About fifteen castellan families dominated the local aristocracy—there was no count after about 925, a small number for such a large province. Most descended from established families of the later Carolingian period, hence Devailly confirms the recent views of K. F. Werner and Genicot about the continuity of the early feudal aristocracy. As a result the transition to the feudal society of the eleventh century came about in relative peace, not in anarchy. Devailly attributes relatively little importance to the building of castles in Berry, maintaining that already established families first constructed them. Yet he is able to say very little about the dates and precise circumstances of their construction. One striking con-

clusion is that most of the older castellan families had died out or been replaced by the early thirteenth century, a sharp contrast to the continuity of comparable dynasties in the Nannais and the Maconnais. Again there are almost no details on this development. Knights begin to appear in the charters of Berry around 1025, just about the same time as in the Maconnais and Poitou, and they formed a social group quite distinct from their noble lords, the castellans. Many knights succeeded here as elsewhere in gradually pushing their way into the aristocracy in the twelfth century. Devailly's pages on them would have profited from more information on the total number of such families, their geographical distribution, and the like, if indeed the sources would permit it.

The sections on the Church are some of the best in the book, especially that on the Gregorian movement, where one finds a concreteness of illustration and depth of documentation missing on the aristocracy. The same qualities distinguish his two chapters on the clearings and rural expansion movements in the twelfth and thirteenth centuries. In this regard I would have welcomed a brief description of settlement patterns in Berry along the lines of André Déléage in his book on Burgundy. A map showing hamlets, villages, and parish churches would have provided a very useful background to the text. On the other hand the book benefits from a number of excellent maps and charts. As examples I mention the map on page 564 that shows the distribution of crops, meadowland, and vineyards in the province, or the one on page 571 that gives the circulation zones of the six different types of money used in the province around 1200.

What does this book add to our knowledge of the evolution of medieval French society? There can be no hesitation in saying that in its broad outlines it confirms conclusions already reached elsewhere rather than offering any startling new hypotheses. Medieval Berry differed relatively little from other French provinces of the time. But to say this is not to depreciate the value of this book or of other similar enterprises. Enough regional monographs of high quality have been written in recent years to reduce seriously the likelihood of radical new discoveries. The role of such studies now is to bring forth new evidence on the many disputed questions of the day and thereby to confirm, disprove, or modify current hypotheses. This Devailly has done well.

GEORGE BEECH

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A. CHÉDEVILLE. *Chartres et ses campagnes (XI^e-XIII^e s.)*. (Publications de l'Université de Haute-Bretagne, 1.) Paris: Éditions Klincksieck. 1973. Pp. 575. 88 fr.

André Chédeville has written a definitive monograph on the social and economic development of the medieval Chartrain. Having described the composition of this region—consisting of three areas: the valleys of the Eure and the Loir, the grain-producing plain of the Beauce to the east, and the less fertile hills of the Perche to the west—he discusses the way in which it was settled, how it was cultivated, and how social relations were created in the process. The second part of the book consists of an analysis of the social classes and the exercise of authority. The final part is devoted to the physical, social, and economic development of the city of Chartres. Throughout, his treatment is based mainly on the meticulous exploitation of archival sources, particularly cartularies.

The growing population of the eleventh and twelfth centuries produced settlers to bring new land under cultivation, usually under the cooperative sponsorship of lay and ecclesiastical lords. The Chartrain shared in the general revival of Europe, but because Chartres was not located on any major thoroughfare, it served only local economic needs and never became an important commercial, industrial, or financial center. Consequently when the arable had reached its maximum extent in the mid-thirteenth century, the Chartrain had reached the limits of its development. The population of the area and the size of the city increased only slightly in the next five hundred years.

Chédeville's analysis of social groupings and terminology offers a wealth of new information, but no surprises. It mainly confirms that the divisions among the *grands* on the one hand and the *humbles* on the other show many local and chronological variations. Like other studies of this sort, the book tells virtually nothing about individuals but offers much about social functions—for example, the role of rural mayors and forms of credit available at the time. The sources are apparently uninformative about feudal relations and political administration in the area, but Chédeville finds that the authority of the count remained strong throughout the period, giving legitimacy to the power exercised by castellans and comital officials and retarding the development of communal institutions in Chartres.

The book is clear, well organized, and based on scrupulous scholarship. It is supplemented with charts, maps, photographs, and an an-

notated bibliography. Chédeville has completed another section of the mosaic of medieval Europe's social and economic development.

JOHN C. MOORE
Hofstra University

M. R. MORGAN. *The Chronicle of Ernoul and the Continuations of William of Tyre*. (Oxford Historical Monographs.) New York: Oxford University Press. 1973. Pp. 204. \$14.50.

The importance of the collection of Old French chronicles known as the *Continuation of William of Tyre* or *Estoire d'Eracles* and the account usually designated ever since the edition of Mas Latrie in 1871 as the *Chronique d'Ernoul et de Bernard le Trésorier* has long been recognized by historians of the Crusades. The various manuscripts have been studied, classified, and edited. There has, however, remained doubt, first, as to the relation between the first part of the continuation (that ending ca. 1231) and the Ernoul-Bernard chronicle and second, the precise roles of Ernoul and Bernard in producing the chronicle, one version of which, the *Chronique d'Ernoul*, mentions Ernoul by name and the other omits it. For the historian there has also been the problem that the first part of the Ernoul-Bernard chronicle presents instead of simply an Old French translation of William of Tyre's *Historia*, a brief summary of events that includes material not found in William of Tyre.

Dr. Morgan has approached these and related problems with the twofold purpose of clarifying the structure of the first group of the continuations and determining insofar as possible the original form of Ernoul's chronicle. Since her main concern is the relation between the various texts of the continuation, which differ in often confusing ways, she has followed the generally accepted manuscript classification system and the judgment of editors in selecting a representative manuscript for each text. One important exception is the choice of MS Saint Omer 722 for the Ernoul-Bernard chronicle. This was not known by Mas Latrie. For the text published by Mas Latrie she uses the term "the *abrégé*" in order "to distinguish it from the continuation proper . . . and from the other short chronicle, the *Estoire d'Oultremer*."

It seems clear that Ernoul was in fact a servant of Balian of Ibelin in 1187, as the chronicle states, and that he later became an important figure in Cyprus. If not a scholar, he was certainly a well-read man and may well

have known Latin. In addition to his later political prominence, indeed perhaps because of it, he was well versed in the legal developments that distinguished the early thirteenth-century culture of the Latin orient. Morgan concludes, nevertheless, that the original *Chronique d'Ernoul* is not now extant but served as material for both the continuations and the *abrégé*. In short, his name should be associated with an original account now lost of the events before 1197.

Bernard le Trésorier, she feels, was definitely associated with the Monastery of Corbie. "His contribution as an original author is . . . very small, but he is the compiler of the 1232 *abrégé*. . . . To Bernard belongs the *travail d'ensemble*." It is his name, not Ernoul's, that should stand as the head of the work.

The author's major contributions, therefore, have been to identify convincingly the roles of Ernoul and Bernard and to reduce to considerably greater order than hitherto thought possible the relations between the various texts. She also has a suggestive chapter, "The Place of the Continuations in Old French Literature." Whether viewed as historical source material or as literature these texts stand out among the few cultural contributions of that unique, French yet also in a special way oriental, civilization of the Latin East.

Morgan's monograph is manifestly the result of painstaking, detailed, often meticulous examination of the sources and all secondary material, historiographical and literary. It will be indispensable for the student of the Crusades and significant for the student of thirteenth-century vernacular literature. There are two appendixes and a bibliography.

MARSHALL W. BALDWIN
New York University

PIERRE-CLÉMENT TIMBAL, with the collaboration of JOSETTE METMAN and HENRI MARTIN. *Les obligations contractuelles dans le droit français des XIII^e et XIV^e siècles d'après la jurisprudence du Parlement*. Volume 1. (Centre d'Étude d'Histoire Juridique.) Paris: Éditions du Centre National de la Recherche Scientifique. 1973. Pp. 553. 128.40 fr.

Professor Timbal has adapted the method of presentation used in his earlier study of the Hundred Years' War to the much more technical subject of contract in French law, 1250-1400. In rigorously legal-brief form, he studies the contingencies of obligation by repeatedly asking the question: What did the Parlement

de Paris do when confronted by the various practical aspects of litigation in matters touching contract? The substance of his answers is provided by 141 cases reproduced in their entirety and 463 cited cases from the Parlement registers. The "jurisprudence" of his title is precise, for he does not propose to analyze the content or diplomatic structure of contracts as they exist in chancery and notarial registers. His definitions and qualifications are as rigorous as his system of exposition, and the result in this first of two projected volumes is a thorough and interesting treatment in two parts of the validity and effects of contractual obligations, although it should be explained that the second part dealing with effects is largely concerned with cessation or extinction of obligation.

Roman, canon, and customary law all had premises to offer on contract. Timbal has studied the reinforcement, rejection, and modification of those premises by the Parlement in its daily work and frequently indicates the significance of this for the Civil Code. For example, canon law had so pre-empted and determined legal preconceptions of usury by 1250 that the Parlement quickly developed doctrine on this question and maintained it. In the more sophisticated matter of *lésion*, of injury or wrong committed in the stipulation of contractual details, the Parlement took its solution almost entirely from Roman law but formed its doctrine slowly and hesitantly and for the most part after 1350. On the other hand, in the relatively simple matter of existence or nonexistence of obligation, it wrestled with new and different problems throughout the period. In all of this, the Parlement as a law court supplying remedy comes off with an impressive record of fairness, common sense, and a high level of juridical integrity.

Of the three largest sections of the book, treating *lésion*, usury, and money of payment, the latter two have great value and interest for the economic and commercial historian as well as for the student of medieval French law. It is hoped that the author will include in his second volume a glossary of dozens of technical terms some few of which must be obscure even to French scholars.

FRANKLIN J. PEGUES
Ohio State University

A. J. MARTÍN DUQUE et al. *Peajes navarros: Pamplona (1351), Tudela (1365), Sangüesa (1362), Carcastillo (1362)*. (Cuadernos de trabajos de

historia, 1.) Pamplona: Universidad de Navarra. 1973. Pp. 199.

JAVIER ZABALO ZABALEGUI. *La administración del reino de Navarra en el siglo XIV*. (Colección histórica de la Universidad de Navarra, 28.) Pamplona: Universidad de Navarra, Facultad de Filosofía y Letras. 1973. Pp. 422.

On the medievalist market, studies of public finance and taxation are currently enjoying a boomlet. In English alone one thinks immediately of W. Bowsky on Sieneese communal finances, E. A. R. Brown on tax morality, J. B. Henneman and Martin Wolfe on French royal revenues, and R. W. Kaeuper and Michael Prestwich on England. Few such studies venture very far back out of the fourteenth century, preferring the security of its proliferating jungle of documentation to the sparse steppes of earlier eras. These two works on fourteenth-century Navarre join that company—one a modest collection of municipal tariff duties and the other an ambitious survey of governmental machinery, which focuses largely on financial administration. Both are fine examples of Spain's ongoing contribution to this field.

Peajes, the first volume of a new historical series, involves an intriguing methodology. Groups of graduate students have collaborated under their several directors, the authors, to put together a mini-edition of four customs registers (four others in this century's registers have appeared elsewhere as articles, and the remaining six will soon be published). The transcriptions provide a welcome addition to the growing supply of published *peajes*, a genre of source material valuable for many kinds of history. The four indexes, unexpectedly spaced throughout the compilation, are more than ample. The dense introductory essays repay close reading, especially for their statistical analyses and charts.

These relatively inscrutable lists, which do not even reveal whether the given merchandise is exiting or arriving, or what might be its origin or destination, nevertheless drop clues by which to chart Navarre's international economy. Thus in the first list seventy-six of the merchants are Castilians, sixty-nine are Arago-Catalans, eighty-six are from elsewhere in Europe, sixteen are Jews, and six are Moors. The bulk of expensive commodities flowed south from France; half of the most common item, oil, came west from Aragon. The commodity lists offer no surprises and do not capture the imagination like the more diversified *peajes* of Mediterranean Spain (duty lists, not

individual collections as here) recently published by Miguel Gual Camarena. Indeed the reader might profitably prepare for this Navarrese material by perusing that author's discussion, elaborate bibliography, and dictionary. Serviceable data can be gleaned from the present slight symposium for application to economic or social history, Romance linguistics, and cliometrics. Imitated as a tool for graduate instruction, it could revivify dormant departments.

Administración, a doctoral thesis of 1966 finally seeing daylight, is a solid, rather heavy study in the classical pattern. Concentrating on the better-documented second half of the century and proceeding by titled subdivisions within subdivisions (like those Japanese dolls that open to reveal consecutively smaller inner dolls), it makes its steady way through the bureaucracies and functions of political government (73 pages), finance (115 pages), the military (19 pages), and parliament (9 pages), finishing with 30 pages of documents devoted to fiscal-economic matters. Wide coverage, touching every official, tax, and gear in the governmental machinery, of necessity imposes a certain superficiality at any given point.

Unlike the similar work of Ludwig Klüpfel on Arago-Catalonia for the late thirteenth century, or Jesús Lalinde Abadía's tome on procuratorial mechanisms, the ensemble lacks grace. Presenting results as an outline, declining frequently into catalogs, lists, and lexicons, it affects the reader like a raw note system, albeit vigorously organized. What confers a special value on the whole is its continuous roots in the Archivo General de Navarra, each item tied to a manuscript citation. This aspect, of course, is a consequence of and tribute to the monumental *Catálogo* for its medieval finance section by J. R. Castro (his successor Florencio Idoate has just issued its fiftieth volume). *Administración* provides an instructive tour through the archive's administrative data, replete with charts, statistics, maps, and incidental erudition.

ROBERT IGNATIUS BURNS, S.J.
University of San Francisco

THEA BUYKEN. *Die Constitutionen von Melfi und das Jus Francorum*. (Abhandlungen der Rheinisch-Westfälischen Akademie der Wissenschaften, volume 51.) Opladen: Westdeutscher Verlag, 1973. Pp. 122. DM 32.

The Constitutions of Melfi, or *Liber Augustalis*, promulgated by Emperor Frederick II in

1231 have proved nearly as inexhaustible a source of fascination and debate for historians as their imperial Legislator. Ernst Kantorowicz called the Constitutions the first great codification of a state's constitutional law since Justinian, and in part he based upon it his concept of the medieval "liturgical state." It has been called both the first "modern" law code and a veritable *imitatio Justinianea*. On the other hand, C. G. Mor, G. M. Monti, R. Trifone, and others have argued for greater continuity of the Constitutions with the Lombard and Norman past. Over fifteen years ago, a team of scholars, including the author of the present volume, announced collaboration on a critical edition of the Constitutions, an announcement followed by Dr. Buyken's *Das römische Recht in den Constitutionen von Melfi* (1960), which appeared to be the first in a series of studies of major influences on the Frederician code. We now have the second study, along with advice that an edition of the Constitutions based on a Vatican manuscript—not the anticipated critical edition—is in press. While the project as initially conceived was greatly needed, the announced edition will be the first Latin edition in over a century—although Professor James Powell provided an English translation based on the Huillard-Bréholles edition along with a helpful introduction and bibliography in 1971, which pass unmentioned by Buyken—and the two studies of influence Buyken has produced to date are most useful. In the present volume she examines the influence of Norman-Frankish customary law—especially feudal and administrative—on the Constitutions of Melfi. Both of her books are intended for specialists: they are based on a lexicographic method that emphasizes legal terminology (*Rechtswörter*) and the use of the same or similar terms and words in different works. It is a detailed and complex process, and it offers both profit and peril: borrowings, filiations, and influences can sometimes be demonstrated with clarity, but the method can also confound similarity with influence (they may not be the same thing), and if too ambitiously and broadly applied the method can, as Walther Holtzmann warned, be used to "demonstrate similar influence in almost any Latin text of the Middle Ages." Buyken has shown greater restraint in this volume than in her last, as well as greater familiarity with recent literature, and here demonstrates the essential continuity of Frederick's code with preceding Norman legislation, especially in matters of feudal custom, the peace, government, and administration.

Frederick was truly heir to the Norman eclectic genius, and his Constitutions were descendants of the Assizes of Ariano issued by Frederick's grandfather Roger II in 1140. Buyken's study does not make use of contextual evidence to substantiate her findings, but the principal manuscript of King Roger's Assizes, Bib. Vat. Lat. 8782, tends to support her lexicographical arguments. It is a late twelfth-century codex that reveals a juridical tradition in southern Italy extending unbroken from the Lombard Edict of Rothari in 643 through subsequent Lombard and Carolingian legislation and the resurrected Institutes of Justinian to the Assizes of Ariano. This manuscript reveals the tradition known to Piero della Vigna and the other jurists who assisted Frederick in the formation of his "new" code. Despite its brevity, Buyken's book is thick in texture and contains nearly a thousand footnotes; a table of contents and bibliography would have been helpful.

PAUL H. MOSHER
University of Washington

TETA E. MOEHS. *Gregorius V, 996-999: A Biographical Study*. (Päpste und Papsttum, number 2.) Stuttgart: Anton Hiersemann. 1972. Pp. viii, 114. DM 48.

This second volume of a biographical series on the papacy is the first monograph-length discussion of Pope Gregory V in nearly a century. Teta Moehs justifies a new study of Gregory's brief reign primarily on the basis of extensive additional documentation, especially as made available in volumes of the *Regesta Imperii* edited respectively by M. Uhlig in 1956 and H. Zimmermann in 1969. She takes the moderately revisionist position that this first German pope was considerably more independent of his imperial cousin and patron, Otto III, than has been recognized.

Gregory V emerges here as a pontiff well aware of Church tradition and of the potential in his office. Moehs stresses Gregory's concern from the outset with reasserting papal prerogatives and moral authority in Western Christendom, a policy reflected most obviously in a vigorously cultivated role as "protector of the monasteries." Thus he conferred exemptions and immunities from local bishops and nobles on monastic houses throughout Europe and maintained a close friendship with the Abbot Abbo of Fleury, a leading advocate of monastic independence from episcopal control. Above all, Gregory's active support of Cluny through grants of privilege—the first pope in this—

can be seen to have furthered considerably the effectiveness of that great reform institution.

But Gregory's exile from Rome by a turbulent local faction ended the promising first half year of his reign and began the rapid erosion of his position. When he returned to Rome early in 998 with a vengeful Otto III, Gregory had clearly become the emperor's man, another instrument toward realizing Otto's dream of a *Renovatio imperii Romanorum*. Otto then presided with Gregory at synods whose decrees served the imperial will more than the papal. The author, however, goes beyond her sources in claiming so firmly that Gregory was henceforth an unwilling accomplice to the imperial cause. She also constantly uses the phrase "must have" to make inferences where "probably" or "likely" would often be more appropriate in light of the fragmentary evidence.

This sober account makes clear, nonetheless, that Gregory V's pontificate, though abortive, foreshadows in important respects the Hildebrandine era of papal revival in the following century.

DONALD SULLIVAN
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CHARLES M. DE LA RONCIÈRE. *Un changeur florentin du Trecento: Lippo di Fede del Segna (1285 env.-1363 env.)*. (École Pratique des Hautes Études—VI^e Section. Centre de Recherches Historiques. Affaires et gens d'affaires, 36.) Paris: S.E.V.P.E.N. 1973. Pp. 277.

In an age when only princes and saints qualified for the order of biography, Lippo di Fede, money changer, was not even a petty clerk in one of the big banking houses of the day. Starting with a small inheritance, his wife's dowry, and a purse full of florins, he went into currency exchange in 1314 and for the next nine years, operating alone, trafficked mainly in regional Italian coins and lent money. Now and then he acted the loan shark but he also lent at little or no interest. His tricky exchange manipulations were most successful during an interlude (1315-18) when Florentine silver currencies were especially unstable. Drawn, like many a Tuscan, by the solid smells of land, Lippo used his profits and skills to buy out some peasants at Pontanico, just outside Florence, and he held on to those lands through thick and thin. Realizing that he operated at too modest a level to profit in the changed monetary environment of Florence, he abandoned wife, home, and farms in 1323 to seek his fortune in France. There for thirty

years, at Pontoise and then Paris. he engaged first in money changing and usurious lending, then in trade. Although his first years abroad were profitable, he fell on bad times again, dropped from sight in 1334, and reappeared in Paris in 1350, henceforth to rely upon money sent to him by his sister. "Naked and without britches," as he was later to be taunted by his second wife (the first having died in 1331), he returned to Italy in 1353. He spent his last years trying to live off a few rents and the produce from his lands, borrowing money from a cousin, and bitterly regretting his late marriage in 1354 to a much younger woman, who claimed finely "*chel ciesso dovella chacava era più bello chio nonavea la boccha mia*" (p. 245).

In fixing the outlines of this man's life—the major source is Lippo's unpublished business diary—M. de la Roncière has produced a work of the first rank. He puts his man squarely into the world of small money changers, changing economic circumstances, city life (Florence), and village life (Pontanico). The book is severely empirical and yet imaginative too. There are charts enough and graphs, an easy handling of knotted subtleties, and a persistent attack on major questions. We look through Lippo into the life of the times, but where his ledger fails, he is presented to us in the life of the times through a *richesse* of other sources. In the process, we also find the author revolutionizing our view of the rural life around Florence, above all by providing some dazzling close-ups.

LAURO MARTINES
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LOUIS GREEN. *Chronicle into History: An Essay on the Interpretation of History in Florentine Fourteenth-Century Chronicles*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1972. Pp. vi. 177. \$13.95.

Mr. Louis Green, senior lecturer at Monash University, has analyzed the contents of five chronicles written by Florentine writers of the fourteenth century: Giovanni Villani, his brother Matteo, Marchione di Coppo Stefani, the Anonymous whose *oeuvre* had once been attributed to Minerbetti, and Goro Dati. Green's is a rather traditional approach to intellectual history, the narrative falling somewhere in between an *explication de texte* and a more general analysis of certain characteristics that strike the author as interesting. Through the analysis of these five works,

Green strives to gain an insight into the changing character of the Florentine *Weltanschauung* of the *trecento*. Starting with Giovanni Villani, to the eventual critical edition of whose chronicle he makes a very useful contribution in the book's two appendixes, he suggests that in the early *trecento* there existed in the minds of Florentine intellectuals a close connection between the supernatural and the natural. This connection, which tended to interpret historical events in light of a supernatural, divine schema, was undermined as a result of the mid-century crisis. The Florentines of the second half of the fourteenth century tended to separate the sphere of the natural from that of the supernatural, assigning the cause of change in the former to factors intrinsic to it. This separation of the two realms, which in Green's view was an important precondition to the development of humanism, did not, however, lead to a more rational and secular mentality. Rather, throughout the fourteenth and fifteenth centuries, there persisted in the minds of intellectuals a belief in supernatural forces, not necessarily as determining the quotidian course of human history, but rather now as portents and omens of impending cataclysmic, millennial disasters.

Green's narrative is subtle and often elegant, and it succeeds in throwing some illuminating insights on rather familiar texts. Quite clearly, there are problems with an approach that uses five texts, written over the course of nearly a century, in order to establish the world view of an entire culture. But leaving aside the question of methodology, one must regret the fact that Green chose to write his book without consulting any manuscript sources; in fact, of the five texts that he studied, there exists a good, critical edition only of one: that of Stefani's chronicle. Perhaps in the end, Green's greatest contribution could be that either he, or a reader of his interesting book, will be moved to undertake one of the major tasks still confronting historians of the Florentine *trecento*: the preparation of a modern, critical edition of Giovanni and Matteo Villani's chronicles.

ANTHONY MOLHO
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P. J. JONES. *The Malatesta of Rimini and the Papal State: A Political History*. New York: Cambridge University Press. 1974. Pp. ix. 372. \$23.50.

At long last, Philip Jones's well-known Oxford thesis has appeared in print. The study has not

matured greatly in the quarter century since it was researched and written; in fact, except for a new introductory chapter and some updating of the footnotes and bibliography, the present work seems to be a virtually untouched version of most of the original. Hence this is only the very promising first work of someone who has become an accomplished historian. But even as it stands, the study is of considerable value. The story of the lordship of the Malatesta family over Rimini and other towns in the Romagna has badly needed a detailed scholarly account, and Jones has provided a clearly, if densely, written narrative based on a close study of the documentary and chronicle evidence in the manner of his former adviser, the late C. M. Ady.

But as a specimen of uncompromising *histoire événementielle*, this study lacks the insight, subtlety, and conceptual breadth of a truly great work of history. The theme of the book is the growth of papal power in the northernmost part of the Patrimony of St. Peter through the papacy's use of local lords as its vicars and military captains. In a series of narrative chapters, Jones skillfully shows how mutual interests dictated cooperation between the popes and the Malatesta in the early Renaissance and how the growth of powerful neighbors doomed independent Malatesta rule in the sixteenth century. (In one of his too rare attempts at the epigrammatic, Jones contrasts the "war of the mice" in the Romagna with the "world war" between the great Italian states [p. 110].) As a detailed, factual narrative, this work possesses the strengths and weaknesses of the empirical tradition of English historical scholarship. Jones has an obsession for depicting events only as contemporary evidence permits and for rigorously documenting each assertion and phrase. He excludes any hint of anachronism; he makes no use of the sophisticated techniques or daring hypotheses being developed by present-day Renaissance historians; he is clearly most comfortable and telling when he permits the chroniclers to speak for themselves in their own languages.

As a result, the bulk of this study is a complex account of diplomatic maneuvering and political alliances that will deter all but the hardest readers. And the last two chapters on Malatesta government, which are the most interesting parts, have in substance already appeared in print (*English Historical Review*, 67 [1952] and in E. F. Jacob, ed., *Italian Renaissance Studies* [1960]). With its massive bibliography, valuable appendixes, and authoritative

narrative, this book is a useful work of reference, but it is not the excellent study of the Malatesta that such a skillful historian as Philip Jones is capable of writing.

BENJAMIN G. KOHL
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GHEORGHE CRONȚ. *Instituții medievale românești: Înfrățirea de moșie; Jurătorii* [Medieval Romanian Institutions: Kinship; Jurors]. Academia Republicii Socialiste România, Institutul de Istorie "N. Iorga." Biblioteca Istorică, number 18.) Bucharest: Editura Academiei Republicii Socialiste România. 1969. Pp. 244. Lei 14.

Gheorghe Cronț's systematic and careful study on kinship and jurors in medieval Romania has a dual purpose: first, to explain the specific Romanian character and characteristics of these two medieval institutions and second, to emphasize the contemporary political doctrine of institutional development as a function of specific national conditions.

The main value of the study lies in the clarification, based on an exhaustive investigation of primary sources, of the institution of kinship. Cronț differentiates between personal kinship, characterized by the common need of defense against external threats to the security of the community, and property kinship, based on the securing of personal-property rights by peasant and feudal lord alike. In his view property kinship was gradually abused by the feudal aristocracy for the express purpose of acquiring control over the property of the peasantry. In fact, according to the author, medieval Romanian serfdom was to a considerable extent a function of abuse of traditional property rights guaranteed through kinship.

Cronț's analysis of the jury system is somewhat less original since it reinforces earlier findings to the effect that the jurors in medieval Romania were almost exclusively witnesses for either plaintiff or defendant in civil as well as in criminal cases. The rendering of verdicts was not in the purview of the jurors' functions and responsibilities. The author elucidates several heretofore obscure aspects of the medieval system of justice, particularly the rights of the ruling princes to assign jurors to either plaintiff or defendant and to manipulate the jury system for their own ends.

The author's findings and conclusions are persuasive with respect to the nature and characteristics of institutions. Less persuasive are his arguments in support of autochthonous Ro-

manian origins and evolution of the institutions themselves. Cronț tends to minimize the impact of external prototypes and influences, particularly the Slavic and the Germanic. His contentions may be borne out by such supplementary evidence as he is likely to produce in future studies. The evidence submitted so far, however, is somewhat inconclusive in that respect.

STEPHEN FISCHER-GALAȚI
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EUGEN STĂNESCU, editor. *Nicolae Iorga—Istoric al Bizanțului: Culegere de studii* [Nicolae Iorga—Historian of Byzantium: A Collection of Studies]. (Academia de Științe Sociale și Politice a Republicii Socialiste România, Institutul de studii sud-est europene.) Bucharest: Editura Academiei Republicii Socialiste România. 1971. Pp. 250. Lei 18.50.

In September 1974 Romania's Institute for the Study of Southeast Europe hosted an international conference in Bucharest. Among the participants were Romanian scholars trained by Iorga (1871–1940), founder of the institute and currently considered his country's greatest historian. This thin anthology, assembled by a leading Byzantinologist who acknowledges the youth of the authors, treats these topics which were earlier investigated by Iorga: Byzantine historical viewpoints, pre-Byzantine topics, the Latin East, the Crusades, Turco-Byzantine relations, Byzantine literature, Byzantine art, and post-Byzantine analyses. Each chapter contains a brief French-language résumé, but scarcely sufficient to satisfy readers who wish to determine the precise contents of each chapter. Since so few of Iorga's works, and fewer of recent studies, have appeared in Western European languages, Romania's historians and publishers could facilitate what they are apparently trying to achieve, namely vast foreign awareness of Iorga's historical contributions. Few non-Romanians possess fluency in Iorga's native language. Romania's scholarly output can gain an international audience if it appears in the more widely used European languages.

SHERMAN D. SPECTOR
Russell Sage College

JACQUES LEFORT, editor. *Actes d'Esphigmenou*. Volume 1, *Texte*; volume 2, *Album*. (Archives de l'Athos, number 6.) Paris: P. Lethiellux. 1973. Pp. xiv, 250; 40 plates. 260 fr. the set.

The monastery of Esphigmenou on Mount Athos was founded in the tenth century and reached the height of its prosperity in the early fourteenth when it numbered two hundred monks and enjoyed a revenue of five hundred gold pieces. Its domains, to a total of about a thousand hectares, were mostly concentrated outside the Chalcidice peninsula, between Lake Bolbe and the Strymon River. It is with the story of these domains that the monastery's archives are largely concerned.

The present edition, which replaces that by L. Petit and W. Regel (1906), contains thirty-one documents, nine of which had not been previously published. Five more documents, two of them forged, are relegated to an appendix. Chronologically they span the period from 1034 to 1409 (?), but there is a gap of nearly two centuries between 1095 and 1258/59 (?). The majority of the documents pertain to the fourteenth century. The story that emerges from them is not entirely edifying. Esphigmenou was in continual conflict with its neighbors, the monasteries of Chilandar, Zographou, and Vatopedi, and this conflict over contested property sometimes assumed a violent form. We are told of the good monks of Vatopedi sallying forth with clubs and torches, destroying houses, and cutting down fruit trees. On other occasions forged titles of ownership were produced. Owing to the substantial gap in the archives, it is not clear by what process Esphigmenou, already rich in the eleventh century, built up its possessions: the earliest list of them is the one dated in this edition to 1258/59. There can be no doubt, however, that in the next century Esphigmenou fared very well under Serbian rule: Stephen Dušan's chrysobull of 1346 (no. 22) refers specifically to the monks' good will toward his Imperial Majesty, for which they were duly rewarded. Restoration of Byzantine sovereignty spelled financial ruin, but when the Turks established themselves at Serres in 1383 and Thessalonica in 1387, Esphigmenou managed to recoup some of its losses thanks to the friendly relations it had formed with the conquerors. Document number 30 of the year 1393 (here published for the first time) is particularly interesting in this respect: not only does it shed new light on the occupation of Thessalonica by the Turks, but it also shows us how at this "dark hour" the Athonite monasteries were scrambling to extend their landholdings at each other's expense.

Lefort's edition conforms to the high standards set up by Paul Lemerle and his collaborators. The Greek texts are given in a diplo-

matic edition that may be verified by comparison with the photographic reproductions, and they are accompanied by French summaries as well as by chronological, prosopographical, and topographical discussions. The situation and extent of the monastery's estates are fully treated and illustrated with tables and sketch maps. This is an excellent piece of work for which all students of Byzantine history will be grateful.

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MODERN EUROPE

DAVID KUNZLE. *History of the Comic Strip*. Volume 1, *The Early Comic Strip: Narrative Strips and Picture Stories in the European Broadsheet from c. 1450 to 1825*. Berkeley and Los Angeles: University of California Press. 1973. Pp. 471. \$35.00.

English lacks a word for the almost entirely uncomic prints described in this fascinating book. Kunzle defines "comic strips" as sequences of four or more separate images with subservient text, published for moral or topical propaganda. This omits the straight news reporting in pictures that began with prints like Dürer's *Monstruous Pig*, a subject well worth a separate book. Kunzle's scope is best conveyed by quoting his table of contents: "Religious Propaganda (c. 1450-1550)"; "France and the Netherlands (c. 1560-1620)"; "The Thirty Years War (1618-48)"; "The Dutch Republic (1653-89)"; "England (c. 1605-1710)"; "Private Crime, Public Execution"; "Vices and Follies"; "Marriage"; "Rakes and Harlots"; "Hogarth"; "Narrative Art after Hogarth"; "The Caricature Comic Strip (1787-1826)"; and "Narrative Art on the Continent." It is at first startling to have the book end with Goya's *Caprichos* and *Diasters of War*, but the unusual context brings out a latent and important meaning in these familiar works of art. The propaganda in this volume only incidentally and occasionally conveys the actual look of the times, but it is a unique document of the vituperative passions that forced the events into being.

The coffee-table size of the volume suggests just another parade of pretty pictures, but this ample format is needed to clarify the details that crowd meaning into these preaching pictures. The reproductions often make the darks muddy and impacted, either because the half-tone plates were etched for too much contrast,

or, more probably, because clear photographs could not be made at the European historical societies where most of the originals are housed. Kunzle provides a clear description of the main collections and a comprehensive index to his outstanding and illuminating study.

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JOHN H. LANGBEIN. *Prosecuting Crime in the Renaissance: England, Germany, France*. (Studies in Legal History.) Cambridge, Mass.: Harvard University Press. 1974. Pp. ix, 321. \$14.95.

In the long debate over the autonomy of English legal development, J. P. Dawson recently argued, in his *History of Lay Judges* (1960), that the adoption of inquisitorial techniques by the Chancery owed nothing to the influence of Roman canonical procedure. Professor Langbein has now performed a similar service for the indigenous development of common-law criminal investigations by justices of the peace in the sixteenth century. Two Marian statutes of 1554 and 1555 required J.P.'s to conduct a brief preliminary examination of suspected criminals and their accusers, write a summary of the proceedings, and present it to the itinerant judges at the next assizes. While W. S. Holdsworth and others have taken these statutes to represent a more or less conscious attempt to model English criminal procedure on "those new and effective [Roman canonical] codes of procedure" lately appearing in Continental states, Langbein presents considerable evidence to the contrary.

Although the statutes unwittingly helped to transform the J.P. from a trier into a prosecutor, says Langbein, their purpose was to elicit an examination much less rigorous and systematic than called for by Roman canonical procedure, much more geared to the preservation of the petty jury as a trier of fact. The statutes can be adequately explained in terms of English experience, for both by legislative authority and by reason of exigency, J.P.'s were already conducting and recording preliminary examinations before 1554.

Not only was the influence of Continental codes unnecessary; it would also have been difficult to Anglicize. In the second half of the book, Langbein analyzes the German *Constitutio Criminalis Carolina* of 1532 and the French royal ordinance of Villers-Cotterets of 1539 (relevant parts of which are translated in the appendix), two important codes that could

have provided Roman canonical inspiration and example for the Marian statutes. In both countries, however, *Inquisitionsprozess* long antedated the sixteenth century. Langbein describes its development and shows that neither ordinance introduced the kind of new procedure Holdsworth assumed, but rather that they regulated certain abuses and difficulties in current practice. The author feels that since they addressed problems peculiar to their respective countries, neither of them could have been useful to the drafters of the Marian statutes. Continental influence on England may not yet be disproven, but the burden of proof seems now to be on its proponents.

In the process of explaining why the differences among these prosecutorial systems are as important as any broad parallels in inquisitorial procedure, Langbein has produced an enlightening comparative study of late medieval and Renaissance criminal prosecution. The uninitiated reader may find it difficult to piece together a comprehensive picture of English procedures, since the author presumes considerably more knowledge on the part of his readers in this area than in his treatment of Germany and France. The curious may wish for a more comprehensive treatment of the subject than the author's primary interests allowed and will also regret the absence of a bibliography. The stylist may find the prose somewhat cumbersome and repetitive at times. But the grateful must thank Langbein for a rewarding and much-needed study.

RICHARD R. BENERT
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DONALD NUGENT, *Ecumenism in the Age of the Reformation: The Colloquy of Poissy*. (Harvard Historical Studies, volume 89.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xi, 258. \$14.00.

It is disagreeable to be negative about a book motivated by the best precepts of unbiased scholarship and Christian charity, one written by a scholar who advises that we "advance Holy Writ in one hand and Herodotus in the other." For Professor Donald Nugent clearly is engaged through his researches into the attempts at Christian union during the Reformation and Counter Reformation in learning from sixteenth-century failures how ecumenism might succeed in the twentieth century. But admirable intent and careful scholarship are not sufficient to make this study really useful, either as history or as advocacy.

With impressive scholarship and thorough impartiality, Professor Nugent constructs a fairly readable narrative of the last serious attempt at reconciliation between the rival religious allegiances in the crisis of the sixteenth-century Reformation. The motivations and policies of Pius IV, Catherine de' Medici, and the enigmatic Charles de Guise, the cardinal of Lorraine—before and during the Colloquy of Poissy—are examined in depth and with sympathy. Nugent expresses what he feels is his unique position vis-à-vis his predecessors, that religious schism in the sixteenth century should not be regarded as normative, and, as a result, he tends to play down considerably the relevance of theological differences between Catholics, Lutherans, and the Reformed Christians. He justifies his effort in the belief "that an ecumenical age best understands ecumenism."

But it is precisely this assumption of modern ecumenical insight that is most questionable. None will dispute that in the twentieth century Christians no longer feel passionately about theological controversies concerning the Real Presence or liturgical opposition to the use of images. Christians in the sixteenth century, for very good reason, took these matters very seriously indeed, for they were of utmost importance. The modern ecumenical and "irenical" viewpoint, in the name of tolerance and charity, undermines the ability to understand the more contentious past. Accordingly, Nugent expresses too many negative opinions about the Council of Trent, largely because it did not try "to assimilate the Protestant genius." One might ask why the Tridentine prelates and theologians should have made greater efforts at accommodating the Protestants. Probably they understood all too clearly their differences with the Protestants and took these differences quite seriously, more seriously than French *politiques* with other insights and other purposes. To Professor Nugent's ecumenism one should apply the corrective of Yves Congar's *Vraie et Fausse Réforme dans l'Eglise*, a study much more historically aware of the relevance of the divisive theological issues of the Reformation.

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HAYDEN WHITE, *Metahistory: The Historical Imagination in Nineteenth-Century Europe*. Baltimore: Johns Hopkins University Press. 1973. Pp. xii, 448. \$15.00.

This is a daring, ingenious, and sometimes bewildering tour de force. White has produced a profoundly original "critique of historical reason," based not upon the usual fare of idealist metaphysics or the logic of predictive science but upon linguistics—a discipline that may become the *novum organon* of the twentieth century. The author presents a unified field theory of history, which takes its departure from the linguistic structures and figurative language implicit in the historical writing of the great practitioners—Michelet, Ranke, Tocqueville, Burckhardt—and theorists—Hegel, Marx, Nietzsche, Croce—of the "classical age" of history.

The novelty of the work lies not with its components but in their systematic combination and deft application to concrete issues. In fairness it must be said that White's style of exegesis is almost impossible to recapitulate in abbreviated form; one must see it at work. He acknowledges his debt to structuralism, the typology of explanations of Stephen Pepper, the literary criticism of Kenneth Burke and Northrop Frye, Karl Mannheim's sociology of knowledge, and above all, Vico's "new science" and its vision of history as a cycle of consciousness rooted in poetic tropes and figures of speech. From this formidable arsenal White has fashioned a "poetic logic" of historical discourse that enables him to cut across (or below) the conventional categories and schools of historical thought.

The method is uniformly and unabashedly formal: White asserts that the historian confronts his data in a manner akin to that by which a grammarian approaches a new language. The historical work consists of various manifest and latent "levels of engagement": esthetic, epistemological, and ethical—but all patently linguistic in nature. The historian must employ a mode of emplotment—Romantic, Tragic, Comic, or Satirical; a mode of explanation—Formist, Mechanistic, Organicist, or Contextualist; and a mode of "ideological implication"—Anarchist, Radical, Conservative, or Liberal. Internal affinities and homologies among these modes constitute the interpretive strategy or "style" of the work. The strategies can be reduced to four "linguistic protocols," corresponding to Vico's four master tropes of Metaphor, Metonymy, Synecdoche, and Irony. These tropes provide the "deep grammar" of the historical account.

History is not a realistic transcription "wie es eigentlich gewesen ist" but a linguistic construct ("verbal icon") of figures of speech en-

tailoring vast but largely hidden assumptions. History is not *mimesis* but *poesis*. White's thesis plumbs the paradox implicit in the two senses of "literal" conveyed in the notion of a literal past: we must perforce think "in terms of our terms"—a self-evident but highly unsettling observation (White cites Nietzsche: "Our science is still the dupe of linguistic habit"). In delineating four different styles of realism, White shows that their standard of objectivity is defined by internal relations among the levels of engagement: there is no historical *Ding an sich*. Ranke's history is no more objective than Croce's, any more than the German language is "truer" than French; they are simply and irreducibly different systems of discourse.

White has taken considerable pains to avoid systemmongering, but his analysis suffers from a certain hardening of the categories. The solemnly upper-case concepts confer a somewhat vatic quality to the work. Although he admits that the best thinkers tended to mix their metaphors and figures, his tracing of homologies tends to assume an almost ritualistic predictability. We are not obliged to take at face value the historicist claims for the "diversity" and "individuality" of things, but in White's *bal démasqué* the surfaces get lost in the deeper paradigm. The tendency to see Irony lurking behind every post is sometimes more bothersome than illuminating. The resolution of dialectics into a trope—for example, in Marx's analysis of the riddle of money—is elegant but ultimately unpersuasive. White's occasionally arcane coinages—"motifcally-encoded," "de-ideologized," "de-naming"—turn the latent level into plain archetypal murk. One might also be led to challenge his purely formal and ultimately reflexive model of language. To use the structuralist terms, historical discourse is *parole* as well as *langue*: it has semantic reference to an experienced world in addition to syntactic structure. "Discourse is not life"—as the structuralists never tire of reminding us—but that does not make it nonrecitative music or symbolist poetry. However falteringly or obliquely (that is, metaphorically), historical discourse concerns itself with real existence as well as formal coherence. One wonders at White's wholesale adoption of formalism, especially in light of his own careful treatment of the objections of Hegel and Croce to precisely this position.

White avows that his book is framed in the Ironic mode—appropriate to a discipline, and

epoch, which has lost its customary certainties and "historical faith." The reader cannot fail to recognize that his perspective is the residual outcome of the very doctrinal antagonisms toward which it is deployed. But there is a visionary as well as critical thrust to his thesis: after indicting academic historians for their "theoretical torpor" and complacent consensus model of historiography, he suggests that history, if conceived mythopoetically, can change the world as well as interpret it. There is a position "beyond Irony" that furnishes the grounds for a new historical consciousness liberated from its old habits and shibboleths. White has provided a comprehensive theoretical framework that transcends the *cordon sanitaire* between "history proper" and the various forms of philosophy of history. Despite a few dark and tight corners, this impressive synthesis casts a very new light.

MICHAEL ERMARTH
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MICHAEL GLOVER. *The Peninsular War, 1807-1814: A Concise Military History*. Hamden, Conn.: Archon Books. 1974. Pp. 431. \$16.00.

Michael Glover has long been recognized as an authority on the military history of the age of Wellington. He is also known as a writer of outstanding narrative skill. In all of his books, particularly *The Legacy of Glory and Wellington as Military Commander*, he has shown a remarkable ability to marshal facts and state clearly his conclusions. In *The Peninsular War, 1807-1814: A Concise Military History*, he has written another superb book of sure-footed scholarship and sustained power.

The Peninsular War begins with a survey of Europe in 1807 and a summary statement about the arms and men in Spain. Then the curtain rises, and the action begins as "the lure of the dream" leads Napoleon to disaster. The titles of the sections suggest the content: "The French Initiative, October 1807-May 1809," "The War in the Balance, June 1809-December 1811," "The British Initiative, 1812," "The Liberation of Spain, January-September 1813," and "The Invasion of France, October 1813-April 1814." Each chapter contains a series of footnotes, excellent maps showing battle plans and details of actions, and well-selected illustrations. There are six appendixes containing bibliographical notes, a list of British regiments that took part in the Peninsular War, the staff of the British army in the Peninsula,

the commanders of the French army in Spain, outlines of the organization of the French army at various periods of the war, the orders of battle of the British army at the main battles, and a good bibliography.

Throughout the pages of the book are vivid descriptions of maneuvers, battles, and sieges; details of weapons, tactics and strategy; the tough tasks of communication and transport; army plundering; the inefficient and easily rattled Spanish and Portuguese officers; the abominable weather; the blood and agony of conflict; and the terrible pathos of the simple statements of the ordinary soldier on the edge of death. Apt remarks and quotations leap and shine on almost every page. Here, for instance, is one of Wellington's sentences that Michael Glover might well have put at the front of his book: "I will tell you the difference between Soult and me: when he gets into a difficulty, his troops don't get him out of it; mine always do" (p. 306).

GOLDWIN SMITH
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ERNST ZIEGLER. *Jacob Burckhardts Vorlesung über die Geschichte des Revolutionszeitalters in den Nachschriften seiner Zuhörer: Rekonstruktion des gesprochen Wortlautes*. Basel: Schwabe & Co. Verlag. 1974. Pp. 648.

A considerable—in his own intention, the predominant—part of Jacob Burckhardt's academic activities consisted in the lectures he offered, through nearly half a century, at his University of Basel: and it is significant that two of his most famous works, the *Weltgeschichtliche Betrachtungen* and the *Griechische Kulturgeschichte* are in fact posthumous publications of lectures. One more of his most popular courses, repeated twelve times from 1859 to 1881, which covers a field of Burckhardt's particular concern and insight, has now been made available through Dr. Ziegler's reconstruction: the *Geschichte des Revolutionszeitalters*. It consists of three major sections centering around the stages and crosscurrents of the French Revolution of 1789, preceded by the analysis of the European scene in the second half of the eighteenth century and followed by the career of Napoleon to the zenith of his power.

Quite a few of his notes for this course, preserved among the vast amount of Burckhardtiana in the *Staatsarchiv* of Basel, have been selected by Emil Dürr for the *Historische Fragmente* in volume 7 of Burckhardt's *Werke*

and recently analyzed in volume 5 of Werner Kaegi's *Biography*. Considering Burckhardt's repute as an unsurpassed master of the spoken word, Ziegler goes beyond these notes in his successful attempt to establish, at least approximately, the lecture on the Age of the Revolution as it was presented to the audience by making use, for the first time, of the notes taken by the attending students. It is fortunate that ten of these notebooks survive, and five of them even cover the same semester (1867-68); this is important, since, even with Burckhardt's mastery of oral presentation, certain variations from year to year were unavoidable. The painstaking deciphering of these notebooks, in part written in antiquated stenography and reproducing the language of the lecture with a varying degree of exactness, and the concomitant comparison have made it possible to reconstruct the essential form of Burckhardt's lectures, confirmed also by numerous parallels in both style and thought with his utterances in his other writings and correspondence.

While it is obvious that a century of additional research and the experience of a more violent age of revolution has invalidated some of his statements, his ability in describing and analyzing the behavior of both individuals and masses in situations of crisis stands out unimpaired. So does his comprehensive view of the entire Western world, from Russia—whose increasing *Drang nach Westen* he keenly observes—to the young power of the United States—with a high appreciation of George Washington. The tragedy of history is enlivened by his striking pictures of the leading personalities, from the protagonists of the Revolution and Napoleon to Frederick II, Maria Theresa, Joseph II, and Catherine of Russia. In addition to extensive notes and bibliography, the book offers an essay on Burckhardt's appearance and style as an academic teacher, a complete list of his courses at the university from 1844 to 1892, and a detailed description of the students' notes.

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JOACHIM HOFFMANN. *Deutsche und Kalmyken, 1942 bis 1945*. (Einzelschriften zur militärischen Geschichte des Zweiten Weltkrieges, 14.) Freiburg: Verlag Rombach. 1974. Pp. 214.

Inhabiting the dry steppes northwest of the Caspian Sea and west of the Lower Volga, the Kalmucks are a Mongolic people and followers of the Lamaist branch of the Buddhist religion.

From 1935 the region was an autonomous Soviet socialist republic, although it resisted strongly Soviet efforts to reduce the importance of religion, collectivize agriculture, and replace the nomadic, pastoral life of a great number of the inhabitants with a more settled and regimented existence. In the summer of 1942 Kalmykia was caught in the path of the German offensive against Russia. In fact, the two major German spearheads bypassed the region, but Hitler thought it advisable to protect their flanks by dispatching the Sixteenth Motorized Infantry Division to occupy the area. Despite initial fears and suspicions when the Germans arrived, the Kalmucks soon found themselves well treated by their invaders and began to collaborate actively. In consequence, when Soviet forces retook the area, the Kalmuck population as a whole was punished; the republic was dissolved and partitioned, fierce reprisals were taken, and, while some of the population managed to escape westward, the majority were moved into exile in Soviet Central Asia. Not until Khrushchev's speech to the Twentieth Party Congress were moves made officially to rehabilitate the Kalmucks. In 1957 they were restored to their former territories, and in July 1958 the area regained the status of an autonomous republic.

In this short but exhaustively researched study Dr. Joachim Hoffmann has provided a thorough analysis of German-Kalmuck collaboration, placing it within the context of traditional Kalmuck resistance to Russification and the intense popular resentment for Soviet nationality policies. He shows the distortions of recent Soviet history on the subject and describes the care with which the German army built up respect among the occupied population. There were few excesses, local needs and sentiments were wisely considered, and some Germans, like Dr. Otto Doll, even became popular heroes. One terrible exception to this moderation was the mass execution of eighty to one hundred Jews from Elista in September 1942, although, Hoffmann argues, this was carried out by an *Einsatzgruppe* (mobile killing squad) without the knowledge of the Sixteenth Motorized Infantry. There was considerable military collaboration by the Kalmucks, and Hoffmann, primarily a military historian, devotes a large portion of the book to the recruitment of a Kalmuck Cavalry Corps and its role in the fighting of 1943. The weakness of the study, apart from an excessive bias in favor of the *Wehrmacht*, lies in its narrow focus: in a failure to speculate upon the relationship of German soldiers to the avowed

racial ideology of the Reich and a further neglect to examine as a whole Nazi policies toward national groups in the east. In April 1941 Alfred Rosenberg drew up a plan for a careful, pragmatic ethnic policy in the east, accompanied by the granting of religious toleration and the breaking up of collective farms as a means of winning local support. This policy was never tried, for Hitler was not interested in liberating minority peoples or in bargaining with Ukrainian separatists. The German occupation was savage even where villagers, especially in the Ukraine, at first welcomed the invaders. The events in Kalmykia, a less important area where racial ideology was not allowed to interfere with decisions, reveal that a policy of understanding and mutual respect between August 1942 and January 1943 achieved considerable results. Many Kalmucks supported General Vlasov's "army of liberation," which the German leaders were desperate enough to make use of in the fall of 1944. Whether Hoffmann believes that a policy of exploiting the antagonisms between Russians and the minority nationalities could, at any point, have constituted a serious threat to the integrity of the USSR is unclear. That Stalin recognized a threat in such a strategy is demonstrated by the mass postwar deportations of ethnic groups from the areas that Germans had occupied.

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IU. M. SAPRYKIN, *Sotsial'no-politicheskie vzgliady angliiskogo krest'ianstva v XIV-XVII vv.* [Social and Political Opinions of the English Peasantry in the 14th to 17th Centuries]. [Moscow:] Izdatel'stvo Moskovskogo Universiteta. 1972. Pp. 320.

Based on his lectures in courses and seminars at Moscow State University, Saprykin's volume is a Marxist interpretation of the maturation of the sociopolitical ideology of the English peasantry and plebs from John Bull to Gerard Winstanley. As such, the focus of attention is on proving the development of a class consciousness and of a class struggle in the transition period from full feudalism to nascent capitalism. Throughout his discussion Saprykin insists on the independence of the process of the genesis and historical significance of the class character of social ideas in political thought. Although he admits that he treats religious, legal, ethical, and esthetic views of the peasantry

only partially, insofar as necessary to better explain social and political ideas and no more, he does not dismiss them summarily for he clearly agrees that the idea of civil and property equality comes from the concept of the equality of all men as children of God as taken from the Bible. Nor does he insist that any peasant struggle in the Middle Ages was anything but a rare and rather primitively developed manifestation. Support for arguments is derived from a wide range of sources: political and religious treatises, poetry, ballads, chronicles of the time, and references to leading works of Soviet and Western historiography of a later period.

The volume consists of four chapters, by centuries, and an introduction. It contains neither an index nor a bibliography. The major lack in the monograph is the absence of a summarizing chapter of conclusions, although each chapter has its individual summary. A startling contrast to the serious tone and generally careful attention to detail in this book is the unbelievable accumulation of spelling and typographical errors in the English footnotes—for example, a different spelling of "Shakespeare" in each of three successive footnotes on the same page.

Whether the reader agrees with Saprykin or not, his monograph provides an approach usually not evaluated in depth in the training of most scholars of English history.

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P. S. CROWSON, *Tudor Foreign Policy*. (Modern British Foreign Policy.) New York: St. Martin's Press. 1973. Pp. xiii, 288. \$10.95.

In this thoughtful study Crowson begins by pointing out that although defense was the primary motive of foreign policy, those rulers who waged war out of dynastic ambition or crusading zeal met a genuine psychological need in their subjects. He adds that a nation's health depends in part "on its vision of a role in world affairs." The author carefully analyzes the actions of each ruler in foreign affairs. He describes the patient diplomacy by which Henry VII redirected French expansion away from northern Europe toward Italy where it did not threaten England or the Netherlands, and he then forestalled the danger that France might again turn northward by building up English naval power and forming defensive alliances with France's neighbors.

Against these achievements Henry VIII's ag-

gressive policies stand in sharp contrast. Yet Crowson believes that Henry's wars were beneficial in the sense that they tended to forge a close bond between the king and his people. He seems overly generous, however, in claiming that Henry entered war with France in 1543 in order to restore national unity at a time when the country was suffering from religious and political divisions. J. J. Scarisbrick is closer to the truth when he says that English participation in this war resulted from a revival of the king's martial ardor against France. Crowson is probably right, however, in stating that Henry's policies at home and abroad helped Englishmen achieve a greater sense of unity and become conscious of themselves as belonging "to an island state detached from Europe, outward looking and yet defiant of intruders."

He explains clearly the factors that influenced Anglo-Spanish diplomacy during Elizabeth's reign. English statesmen wished to preserve Philip II's political authority in the Netherlands in order to prevent French expansion into that area. They opposed only a Spanish military presence in the Low Countries because it constituted a threat to England's security. Mary Stuart's continued existence as a prisoner in England at first restrained Philip from aggression against Elizabeth, since her overthrow would result in Mary's elevation to the English throne and in the creation of a powerful French Guise empire. (This danger incidentally had caused Philip to befriend Elizabeth during her sister's reign, not consider her execution, as the author suggests.) Spanish policy changed by 1580, according to Crowson, in part because of Philip's reassessment of forces in France where the power of the Guises had been weakened and where there now existed the possibility of a united politique or Protestant nation. He therefore made himself the patron of the Guises and of Mary Stuart, and he became more actively hostile toward Elizabeth.

Though not a work of detailed scholarship like R. B. Wernham's *Before the Armada: The Emergence of the English Nation, 1485-1588* (1972), this book is a competent, well-written survey of Tudor foreign policy. Crowson not only has described the long-term developments but also has made clear the day-to-day perplexities faced by leaders of a small country who realized that "one unlucky error" might bring disaster upon them.

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BARRETT L. BEER. *Northumberland: The Political Career of John Dudley, Earl of Warwick and Duke of Northumberland*. [Kent, Ohio:] Kent State University Press. 1973. Pp. xi, 235. \$10.00.

Of all the traditional villains of the Tudor period, the one who might be considered least likely as a subject for rehabilitation would probably be John Dudley, duke of Northumberland. Since the time of his execution, he has been an exemplar of power lust, of unprincipled ambition, of amoral greed. His attempt to displace Mary Tudor in favor of his daughter-in-law, Jane Grey, was seen as the ultimate evidence of his overbearing pride, an audacious attempt to found his own dynasty. The first major challenge to this enduring and colorful characterization was that of W. K. Jordan, who saw the impetus for the Jane Grey episode as coming not from Northumberland but from a dying king who was fanatical in his determination to save the throne from the papists.

Now Professor Beer has taken several giant steps beyond Jordan and has delineated a personality so transformed that many students of the period will recognize him only with difficulty. Beer's Northumberland is a moderate, ordinary man, a soldier and administrator of less than overwhelming ability who rose to dizzying power by virtue of his unflagging loyalty—and the absence of any conspicuous talent among his contemporaries. He was "perhaps ambitious" but did nothing in his pursuit of place and fortune that would cause any Tudor gentleman to blush. He did not scheme to destroy Lord Protector Somerset; in effect, Somerset destroyed himself by his own folly. The plot to exclude Mary Tudor from the throne is accepted by Beer to be Northumberland's rather than Edward's, not as a piece of supreme arrogance but as "an act of futile desperation executed by a confused and sick man who had lost sight of his own interests." It was so clumsily devised and carelessly carried out that it proves Northumberland "lacked the vigor and enthusiasm of a true Renaissance despot." Throughout the volume Beer credits Northumberland with motives that are at once orthodox and honorable—even his celebrated scaffold conversion to Catholicism Beer accepts as a genuine quest for spiritual solace rather than a last-ditch appeal to the mercy of a Catholic sovereign. And, Beer notes, Northumberland actually passed more reform legislation than any other Tudor administrator in a similar span of time. In this sympathetic

analysis Northumberland becomes an infinitely less sinister figure—but less interesting.

Northumberland may certainly be overdue a scholarly reappraisal; it is hard now to believe in a historic personality totally without redeeming social values. But much of Beer's attitude seems to be the result of tunnel vision—he has focused so narrowly on Northumberland's administrative activities that it is difficult to gain a critical perspective. There is no real attempt here to enlarge the context to a national one, a task that is perhaps impossible so soon after Jordan's volumes, and there is no close analysis of the nature or impact of his power. For example, the assertion that Northumberland was really a reformer is not supported by any careful analysis of his economic and social programs, and it must stand against a strong tradition that he was an economic reactionary devoted to class interests, who restored order and privilege at high social cost. Beer's assumption seems to be that if we followed Northumberland through his days, watching him behave as other men did, we will be persuaded that he was a reasonable enough fellow, though far less compelling than we had always supposed. But Northumberland's stains are too stubborn to be removed with so mild a washing; he needs more exposure to sunlight than has been provided so far.

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ELIZABETH READ FOSTER. *The Painful Labour of Mr. Elsyng*. (Transactions of the American Philosophical Society, New Series, volume 62, part 8.) Philadelphia: the Society. 1972. Pp. 69. \$3.00.

Henry Elsyng, clerk of the parliaments from 1621 to 1635, author of *The Manner of Holding Parliaments in England*, record keeper, antiquary, political advice writer, and historian, earned a high reputation during his lifetime for intellectual ability and painstaking labor. In 1681, some forty-five years after his death, his essay "Of the Judicature of Parliaments" was published as a posthumous piece, supposedly written by John Selden. The attribution was flattering, but Elsyng's personal reputation as a scholar and shaper of parliamentary practice was not fully restored until the twentieth century. Only in 1954 was the important second book of *The Manner of Holding Parliaments* (first published in 1768) meticulously edited by Catherine Strateman Sims. Now, in Elizabeth Read Foster's excellent monograph, Elsyng's

public career has been fully documented and given the careful analysis it deserves.

This book discusses Elsyng's early work as a record keeper, in company with his uncle, Robert Bowyer; his long service as clerk of the parliaments; how he kept his "scribbled books," and how these served as the basis for the Journal of the House of Lords; his innovations in the make-up of the Parliament Roll; his historical treatise on Parliament (with special reference to "War and Peace," "Subsidies," and "Judicature"); his knowledge of, and use of, antiquarian precedents; and, finally, his political advice on how the king might regain his subjects' affections.

Basing her account of Elsyng's career almost entirely on manuscript sources, Foster has written an austere, factual account, without intellectual digressions or biographical conjectures. Bigger books have resulted from less research in original records. Foster's own style is not unlike that of Elsyng, who "hewed close to the line he had set for himself," avoiding the lawyer's brief and the political pamphlet. "Of those parliaments which he served as clerk, he left a solid record in journals, rolls, and notes," Foster concludes, and "of those which he studied in earlier times he wrote a careful analysis, linking past and present with the practical experience of a man who was working in the living institution." Elsyng was not a Selden, but he served with distinction both his country and that select group of educated men who sought to understand historically, and thus to perfect, English institutions.

F. SMITH FUSSNER

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JOHN R. MACCORMACK. *Revolutionary Politics in the Long Parliament*. Cambridge, Mass.: Harvard University Press. 1973. Pp. xiii, 365. \$14.00.

J. H. ADAMSON and H. F. FOLLAND. *Sir Harry Vane: His Life and Times (1613-1662)*. Boston: Gambit. 1973. Pp. 498. \$10.00.

John MacCormack's bold study of revolutionary politics, especially political parties, in the Long Parliament attempts to fill a scholarly gap between Jack Hexter's *Reign of King Pym* (1941) and David Underdown's *Pride's Purge* (1971). MacCormack contends that Hexter's war and peace parties of 1643, which Underdown called "Independents" and "Presbyterians" after 1645, were respectively transformed into radical and moderate parties from 1643 to 1648 and that Hexter's middle group immediately disappeared after John Pym's death in December 1643. Both

the radical and moderate parties adopted the ideals of their predecessors, but the radicals, who were led by Henry Vane, Jr. and Oliver St. John, worked for republican ideals that were opposed by the moderates, who were led by Denzil Holles and Philip Stapleton. By early 1647 the moderate party, which had been in opposition for three years, gained a majority chiefly because of the unexpected additions from the newly elected recruiters. By early 1648 the radical party, whose cracks were "so assiduously papered over in 1644" (p. 7), was split into three groups, Levellers, Cromwellians, and Vanists, because of the radicalization of the extremists under Henry Martin, the "bewildering succession of policy shifts" (p. 308) by the Cromwellians, and the willingness of the "moderate radicals" to accept the conservative principles of the Heads of Proposals. Neither Vane nor St. John could support either Pride's Purge or the execution of the king; for them and their followers the old revolutionary *élan* of the radical party was gone.

MacCormack's adherence to a two-party system for the years 1643-47 challenges entrenched historical opinion to the contrary. Following Hexter's guidelines, Valerie Pearl (*English Historical Review*, 81 [1966]: 490-519), Lotte Mulligan (*Historical Journal*, 12 [1969]: 3-32), and Underdown found evidence for a middle group led by St. John between the two parties from 1643 to 1648. If this middle group is valid for the mid-1640s, then it is possible that MacCormack's radicals were not as numerous or as revolutionary, and his two extreme parties were not as cohesive, as he suggests. There is some indication, however, that MacCormack is not as far apart from the other historians as it may seem. For instance, Pearl and Underdown state that the middle group worked very closely with the war or Independent party. But more important, MacCormack's appendix of 565 members of the Long Parliament from 1644 to 1648 categorizes each member in 1644, 1647, and 1648 as a core, fringe, or latent radical or moderate. Vane and St. John are consistently core radicals except for the first six months of 1647 when they are listed as fringe moderates as well. Although the "fringe" categories suggest an amorphous characteristic for both of the parties, as well as the possible existence of a middle group, they are not helpful in delineating the difference in 1644 between the supporters of Vane, the oligarchic republican, and the supporters of St. John, the limited monarchist. The addition of such categories

as "Martin radicals," "Secluded radicals," and "Core Cromwellians" for 1647 and 1648 may indicate that the radical party was more revolutionary at the end of its life than at its beginning.

MacCormack not only places much less emphasis upon religious issues than on political issues in the Long Parliament, but he also eschews the terms "Independent" and "Presbyterian" as party designations. Both Hexter (*AHR*, 44 [1938]: 29-49) and Underdown have undermined the old view, set forth by S. R. Gardiner, that religious Presbyterians and Independents could be equated with political parties with the same names. Yet Underdown found that the question of a Church settlement dominated parliamentary proceedings in 1645 and 1646 and that during Pride's Purge a typical "revolutionary" was probably an Independent. Once again, however, despite the terminology and emphasis, MacCormack may not be all that far from Underdown. For example, although he states that the radical religious perspective was varied, he observes that the radical leaders were of Independent sympathy and that the army, which was dominated by Independents after 1645, was the mainstay of radical power. Or again, he says that the Presbyterians' constant defection from Holles seriously weakened the moderates, and that from 1644 "an outward enthusiasm for Presbyterianism was henceforth a minimal requirement for the moderates" (p. 311).

Although one might disagree with his party categories, or his assessment of the role of religion, MacCormack's provocative conclusions will force seventeenth-century historians to rethink their own conception of parliamentary politics during the middle years of the Long Parliament.

The Adamson and Folland book on Vane is essentially a life-and-times biography—in some chapters more times than life—that draws upon selected primary and secondary sources. The authors are not primarily concerned with the question of parties in the Long Parliament, although they acknowledge a special indebtedness to Hexter, or with Vane's administrative career with the navy, which is the chief focus of the other recent biography of Vane by Violet Rowe. Instead, they find Vane defending the principle of religious toleration throughout his political career. They do not realize, however, that his "single goal of a free religious society" (p. 333), which they seem to admire, might be in serious conflict with his millennial ideas

about the kingdom of Saints, which they do not seem to understand. Had they familiarized themselves with more of the recent research on Puritanism, they might not have concluded that "millenarian worms were aerating his brain" (p. 385) or that "his writing is infected with theological jargon" (p. 326). The authors perceptively observe about Vane's chief millennial work, *The Retired Man's Meditations*, that "it is always a little frightening when a man of public trust and power fully displays his inner life, for then we realize what fragile pillars uphold our social fabric" (p. 333). Yet it might be added, in Vane's case at least, that it is the only way to truth and understanding.

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DAVID UNDERDOWN. *Somerset in the Civil War and Interregnum*. Hamden, Conn.: Archon Books. 1973. Pp. 229. \$11.50.

In fewer than two hundred pages, Professor Underdown has presented a sparkling narrative of the impact of revolution and reaction on one of the most stable of English county societies in the seventeenth century. The epitaph of one of Somerset's staunch Royalists and pre-revolution grandees, Sir John Stawell, is applicable to the county's society in the Civil Wars and Interregnum: "Post perditam rem familiarem aedium ruinam." Great names of two or more generations' standing in the governance of the county—Poulett, Phelps, Hopton, Stawell, Berkeley, Wyndham—were eclipsed by the newly risen adherents of the new model in state and church—Ashe, Curll, Hill, Burges, Henley, Strode, and Pyne. Even a Horner and a Popham, early supporters of Parliament's cause, were swamped in the exercise of power by the upstarts. It is Underdown's great achievement that by the vehicle of analytical narrative following strict chronology he reveals with clarity and develops with precision the complex tale of division, confrontation, strife, usurpation, innovation, and reaction within the county's political structure upon the impulse of events beyond the county's borders as well as within them. This is not "local history" but national history in microcosm, entirely within the new tradition of local study of national dynamics. An immediately apparent benefit is the unusual segmentation of chronology when the First Civil War is seen from the vantage point of Somerset rather than the Westminster-Oxford meridian, as set down in chapters 2-6.

No less important is the revelation once again of how great and stirring events, the dictates of central government, and change moved or comprehended nationally are sieved through personal ambition, posturing appropriate to the local arena, standing petty feuds, and traditional local ideologies, allegiances, and modes of political action, to fall with varying force and differing influence on the countryside. The irony in Royalist Edward Phelps's defiant assertion to Cromwell in 1653 that his father, Sir Robert Phelps, would have been of "my persuasion" if he were still alive (p. 173) is manifest if one recalls a notion of the 1620s of the type that Sir Robert had been and in 1653 the radical John Pyne was, "popular persons . . . as have exacted an opinion to be thought their countries only freindes." The parallels that exist between Somerset politics—despite the discontinuity in Somerset society—between 1641-60 and 1621-41 are made clear, although implicitly, in this book.

It might appear unfair to tax Underdown with not having done all that I hoped he would with Civil War and Interregnum Somerset. Undoubtedly the limitations imposed by the general editor of the series in which this book appears, as well as the author's proven strength in analytical political narrative, dictated the exclusion of a descriptive analysis of county institutions changing under the impact of revolution and of a detailed analysis of the social changes worked among the politically important in the county. It is a pity that another hundred pages could not have been devoted to these concerns. Somerset, unlike most other English counties, has abundant county records for the study of local institutions in this epoch, and it has attracted a number of researchers. Perhaps most significant, this study comes after the new departure of Alan Everitt's work in Civil War and Interregnum Kent: Everitt's book was raised upon a conception and a methodology that deserves emulation and demands comparison. There are a number of assertions in Underdown's book that depend upon detailed analysis of local institutions and social changes. There is little in these assertions that causes disquiet. Yet the study—and scholarship—would have been richer had the detailed analysis behind Underdown's conception of the county's government and society been made explicit. One hopes that beside this graceful and penetrating narrative Underdown will one day place another, if less comely, volume exposing Somerset's underbrush, the alders and the sedge willows. The oaks and

poplars found here cannot be better drawn, only made more grand by contrast.

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BLAIR WORDEN. *The Rump Parliament, 1648-1653*. New York: Cambridge University Press. 1974. Pp. xii, 427. \$25.00.

The need for a detailed study of the Rump Parliament has been felt for many years, but the shortage of sources, particularly the complete absence of any parliamentary diaries for the period, has caused historians to shy away from undertaking the task. The appearance of Dr. Worden's splendid book has admirably filled the gap. In one of the best studies on the English Revolution to appear in years, he provides a coherent account of the politics of the Rump, a penetrating analysis of its membership, and a sharp revision of many of the stereotypes that have been perpetuated about the parliamentary politics of the period.

Although it draws extensively on the works of others, notably on Underdown's study of Pride's Purge and Williams's unpublished Oxford thesis on Henry Marten, the book is original and learned. It is political history at its best, closely informed by the social and economic questions raised in recent years but refreshingly free from the tenuous general theses that have bedeviled research in those areas. The book itself contains three sections: a study of the membership of the Rump, an analysis of the various efforts at reform, and a narrative account of the Parliament's history. Worden demolishes what was left of the idea that there were coherent parties in the Rump and effectively demonstrates that there is no cause to think "that those M.P.s who continued to sit after the Purge were different kinds of people from those who did not" (p. 25). Moreover, while accepting Underdown's distinction between "revolutionary" and "conformist" members of the Rump, Worden modifies this distinction by noting among the revolutionaries the absence of any common or continuing revolutionary purpose. M.P.s who were revolutionary enough to support the execution of the king did not necessarily favor law reform or religious toleration. In fact, despite its origins, the Rump was a basically conservative body.

Perhaps the two most important contributions of this study are the light cast on Oliver Cromwell and the convincing reconstruction of the last days of the Rump. There has long

been suspicion about the story that Cromwell dissolved the House because of its alleged effort to perpetuate itself. Williams's thesis offered evidence bearing on this point, but Worden has gone well beyond it and demonstrates convincingly that the bill under discussion at the time of the expulsion was for a dissolution to be followed by new elections, not for enlarging the existing body by recruiting. The interpretation of Cromwell, who is seen as asking the impossible of the Rump, whose essentially unrevolutionary character he shaped, is one that no historian of the period can ignore. It is difficult to make sense of Cromwell, but the picture that emerges here is more convincing than that to be found in many biographies.

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G. E. AYLMER. *The State's Servants: The Civil Service of the English Republic, 1649-1660*. Boston: Routledge and Kegan Paul. 1973. Pp. xiii, 484. \$26.75.

Professor Aylmer's sequel to his study of the civil service of Charles I reflects the same meticulous scholarship and careful reasoning that characterized his earlier work. As administrative history of the highest caliber it covers a wide range of topics and employs a variety of historical techniques. Not only does it provide a wealth of information regarding the administrative system and personnel of the Commonwealth and the Protectorate, but it examines in a most provocative way how politics, bureaucracy, and society interacted during the revolutionary period.

Illustrating continuity as well as innovation in a time of rapid political change, Aylmer first surveys the institutional structure of the republic and then explores such topics as entry to office, tenure, payment of officers, and corruption. Much of the evidence presented here, especially that which concerns salaries, pluralism, and the printing of forms, supports Aylmer's contention that a "more modern, professional civil service . . . was taking shape during these years." The author later explains, however, that the fundamental changes necessary to produce a true public service, though envisioned by some pamphleteers, were never implemented.

The most important and valuable section of the book deals with the 1,175 men who comprised the republican civil service. Using both a sample of two hundred officials and a selection of one hundred middle-rank administra-

tors. Aylmer constructs a group biography in which he employs the results of his earlier work for comparative purposes. This establishes some interesting patterns of social and geographical mobility and reveals that the civil service of the republic attracted men of distinctly lower social standing than those who had served Charles I. Some fifty individual case studies toward the end of this chapter add a human dimension to the statistical material that precedes them, but they do not significantly improve the clarity of the collective portrait. On the other hand, the book does not even disclose the identity of the remainder of the 1,175 administrators.

In a chapter entitled "The Impact of the Regime," Aylmer exploits a wide variety of sources in an effort to view his subject from the perspective of the governed rather than the governors. In many ways the historical reputation of the regime benefits from this fair and cautious assessment. The indemnity laws "did not develop into a general *droit administratif*"; the military did not ride roughshod over the liberties of the subject; and despite an increase in taxation, wealth was transferred through office from the traditional elite to slightly wider sections of the population. Of course the central government made its presence felt throughout the country, but local government changed remarkably little, and in general "the effects of republican rule were less marked in matters of policy than in those of personnel."

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FREDERICK BRACHER, editor. *Letters of Sir George Etherege*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xxv, 324. \$15.00.

Professor Bracher is an expert on Sir George Etherege, and his edition of Etherege's letters is obviously a labor of love. Etherege was one of the Restoration literary wits and roués, of whom Congreve and Wycherley were the best-known dramatists, while Rochester (Wilmot) and Buckingham were known particularly for their wit and achieved high position at Court. Few of the group left letters, and their wit survives as remembered bon mots. Etherege is the exception: some four hundred of his letters exist (holograph, autograph, or copy), and Bracher has published about half of them *in extenso*. Unfortunately, most of them are official and date from the duller period of Etherege's life, 1685-89, when he represented England as an observer of the Imperial Diet at Ratisbon.

Etherege was certainly an unconventional diplomat. He accepted the post for the income and to escape his numerous creditors in England to whom he owed large gambling debts. For an inveterate gambler Ratisbon offered only a game (ombre) at sixpence a point. For a womanizer—another of Etherege's weaknesses—the pickings were even poorer. The stolid, virtuous wives and daughters of German diplomats were poor material for entertaining liaisons. The only prize in that line was a Dutch actress on tour, whom Etherege carried off to his quarters. The jilted locals staged an assault to recover the lady—some of them, disguised as footmen, were even injured. There is an account, highly unfavorable to Etherege, by Etherege's Puritan secretary; but the story can also be reconstructed from Etherege's letters.

The battle for the actress is the high point of the volume. Most of the letters are from Ratisbon and addressed to officials and friends in London. The editor spares us the details of the proceedings of the Diet, which Etherege was to report weekly or biweekly to his superior, Secretary of State Lord Middleton. The two were apparently on fairly close terms, and whenever Etherege indulges in a bit of flippancy or wit, the text is given in full. More amusing are the occasional letters to old cronies. Most of them had risen in Court circles, and Etherege hoped to get their help in speeding up the payment of his expenses.

The book contains a few gems, but they hardly sustain so weighty a volume. An article, quoting the juiciest bits, could have conveyed virtually as much without the elaborate paraphernalia of scholarship that Bracher makes use of in editing what is after all a correspondence of minor literary and historical significance.

ROBERT WALCOTT
College of Wooster

JOHN E. FLINT and GLYNDWR WILLIAMS, editors. *Perspectives of Empire: Essays Presented to Gerald S. Graham*. New York: Barnes and Noble. 1973. Pp. xii, 200. \$11.50.

GERALD S. GRAHAM. *Tides of Empire: Discursions on the Expansion of Britain Overseas*. Montreal: McGill-Queen's University Press. 1972. Pp. x, 108. \$6.50.

Gerald S. Graham has been one of the most influential students of the history of the British Empire. If influence is measured in terms of the number of advanced students directed through their doctoral dissertations, Graham can have no rival; and there could be no more than

three or four additional names to be added to his by any other measurement. To select essays from so widespread, large, and able a group of students, in order to present Graham with a *Festschrift* on the occasion of his retirement from the Rhodes Professorship of Imperial History in the University of London, could not have been an easy task, but John Flint and Glyndwr Williams, the editors, have performed it well. With so many to choose from, one may have reason to expect excellent essays, and yet *Festschriften* have a way of being unreadable and often carry the signs of bottom-drawer work hastily retrieved.

This volume shows no such signs for it is consistently interesting, often contains illuminating new insights, and is a genuine compliment—to editors, authors, and—most important—to its recipient. There are ten essays as well as a full list of Graham's writings. One of the essays is an intriguing, short, and allusive discourse on Graham as teacher and historian, written by K. Onwuka Dike, Graham's first student from Africa, now professor of African history at Harvard. An unusual, highly suggestive essay by A. F. McC. Madden of Nuffield, on the relevance of English medieval experience of empire to later imperial constitutional issues, both reminds us of how much value one gains from Madden's works and how much one might wish that he would give us more of them. Madden contends, and proves, that a knowledge of pre-seventeenth-century empire is illuminating and corrective; he also gives the Channel Islands a fuller place in imperial history than we have had reason to suspect.

To suggest that Madden's is a favorite essay is not to diminish the others. A fascinating chapter by John S. Galbraith on the origins of the British South Africa Company gives us a foretaste of his next book. Flint writes freshly on an old subject, the partition of West Africa, and contributes a major piece to the growing debate over the significance of the various theses identified with the names of Ronald Robinson and John Gallagher. Peter Burroughs, in the longest essay in the book, gives us an excellent, consistently interesting examination of the place of the Canadian rebellions of 1837 in British politics, reversing the causal flow of Madden's inquiry. Williams writes with gusto of the early English projects in the South Seas between 1670 and 1750, while E. A. Ayandeke of Jos introduces us to a number of new yet important "visionary nationalists" in precolonial Nigeria. Two nicely matched essays, "The

Imperial Factor in the Liberal Decline, 1880–1885," by Peter Marshall, and "Historians and Britain's Imperial Strategic Stance in 1914," by D. M. Schurman, also provide new insights into old subjects, and both are excellent essays with which the student of post-1870 empire in British thought might begin. Finally George P. Grant in "Ideology in Modern Empires" gives us the shortest essay in the book. Here he attacks familiar demons—the notion of "value-free" multiversities, outbreaks of ideology, the "technicised" nature of the study of politics—while providing an original way of relating R. H. S. Crossman to imperialism. This Floris chocolate at the end of so rich a fare seems appropriate.

Graham's most recent book follows close upon his concise histories of the British Empire, and of Canada, five years after his superb work on British maritime enterprise in the Indian Ocean, and not long ahead—one hopes—of his next volume, which will bring the examination of that ocean in British imperial history further into the last century. The present volume, although what that century might have called "occasional pieces," based as it is on the Reid Lectures delivered at Acadia University in 1969, is a readable, often fascinating, romp through five hundred years of history. Graham, unlike so many recent writers, is prepared to state his own view on the "balance sheet of empire" quite clearly, ending his overview with the judgment "that the peoples whom the Empire-builders once served gained more than they lost from British dominion overseas."

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IVY PINCHBECK and MARGARET HEWITT. *Children in English Society. Volume 2, From the Eighteenth Century to the Children Act 1948.* (Studies in Social History.) Toronto: University of Toronto Press. 1973. Pp. vii, 347–671. \$10.00.

This work constitutes the second volume of a survey tracing the genesis of social concern for children in England. Volume 1, published in 1969, covers the period from the fifteenth to the end of the eighteenth century and traces the foundations of the English child-care system. Drs. Pinchbeck and Hewitt have written a comprehensive, lively, and wide-ranging study of the plight of children in the early years of industrialization and of growing private and public intervention on their behalf.

The title of the volume is somewhat misleading, since the focus is largely on the offspring of the poor and not on the broader per-

spective of middle- and upper-class practices. The authors are primarily concerned with the development of voluntary and statutory provisions for destitute youngsters and in this context treat the illegitimate child, the young worker, the pauper, the orphan, and the delinquent. They describe in useful detail the careers of reformers like Captain Edward Brenton, Mary Carpenter, and Mrs. Nassau Senior, as well as many of the individual charities.

At first, suggestions of widespread public aid faced strong opposition on the grounds that it interfered with parental rights and was subject to abuse. Population growth and urban concentration ultimately combined to multiply the impact of the problem and make general solutions more desirable. There is no doubt that the situation of vast numbers of youngsters was worse than deplorable during the early years of the previous century. Children had few rights before the law in terms of protection and maintenance, adult standards of punishment and labor being generally applied. Legislative reforms paralleled the development of the concept of childhood as a separate and vulnerable stage.

In general, the Victorian era witnessed the introduction of the child-care movement and delinquency legislation, while the next fifty years saw their further refinement and consolidation. Basing their study on a wide variety of contemporary sources, the authors provide a sound analysis of major developments in this field. The chapter "Prevention of Cruelty and Neglect" offers firm evidence that child abuse is not solely a contemporary phenomenon, nor was willful maltreatment always confined to the lower classes. A similar section on emigration schemes offers useful insight into the varying aims and degrees of supervision common to these programs. By providing a coherent survey of the development of services to children, Pinchbeck and Hewitt have made a valuable contribution to the literature of juvenile reform.

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H. ULRICH STAUB. *Staatsmann oder Politiker? Die Repräsentanten der englischen Geschichte des 18. Jahrhunderts.* Wiesbaden: Franz Steiner Verlag GmbH. 1973. Pp. vii, 166. DM 28.

Professor Staub poses an interesting and potentially important question: by what criteria can the statesman be distinguished from the politician? But his ramble through eighteenth-

century British history in pursuit of an answer is finally ineffectual. The study is not a disclosure of new material, for the author has used only secondary sources. It is not a textbook, for it omits a variety of crucial matters. It is not a survey of recent research, for it relies on a most uneven collection of generally older sources (Coxe for Walpole, Rosebery for Pitt, Thackeray for George II), and in its assertions of George III's Bolingbrokean education and Newcastle's control of legions in the Commons it draws on a historiographical world now two generations out of date. Finally it is not an examination of the theoretical foundations of statesmanship, for the introductory chapter that sketches in the political thought of Plato, Luther, Rousseau, and Kant does little more than reduce their complex ideas to the maxim that the good ruler should consult national rather than partisan interest.

Most distressing, Staub never makes clear what distinguishes the statesman. Although he asserts that eighteenth-century Britain was peculiarly blessed with statesmen, his survey of the age provides no persisting themes of action that might be taken to characterize statesmanship. His roster of statesmen is itself unremarkable, save in its high evaluation of Carteret, but a comparison of the reasons behind the inclusion of various figures suggests Staub's problem. Marlborough qualifies because he knew that war against France was vital; Stanhope because he saw the need for an alliance embracing France; Walpole because he recognized the value of peace with France; the elder Pitt because he favored an integrated and unremitting war on France (and abstinence toward America); the younger Pitt because he built alliances against France (and liked reform). Clearly warmaking is sometimes statesmanlike and sometimes not. But Staub does not tell the reader why or when, and one is left with the impression that in the final analysis it is contemporaneous fame and posthumous attention that alone segregate the statesmanly sheep from the political goats.

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ANTHONY BURTON. *The Canal Builders.* London: Eyre Methuen; distrib. by Barnes and Noble, New York. 1972. Pp. 230. \$11.00.

In a way it might be considered easy to write a book on the early canals in Britain. The inherent fascination of the subject; the blossoming of the profession of public engineering; the intriguing methods of construction such as the

locks, the tunnels, the aqueducts, and puddling; and the canal mania have all given Anthony Burton an impulse and momentum that have contributed to his success. Perhaps they have given him a little too much momentum, for, although formally recognizing that canals, tunneling, and pound locks were not really new, he gives the impression of canals bursting upon the country as an entirely new phenomenon. The earlier river navigation improvements and the draining of the fens are certainly very important, if not as spectacular. On the other hand, from Burton's point of view perhaps he is correct, for he is not writing strictly about the canals themselves but about the canal builders (although he falters a little on p. 209 where he says canal building is his subject).

The builders are the meat of his book. The duke of Bridgewater and James Brindley receive more than adequate attention; but when we come to the chief engineers, the resident engineers, and the contractors we are at grips with the practical problems—problems of construction, financing, public relations, and labor relations. And we have, as far as I know, the first real view of the navvies. We see how they were recruited from the countryside, from the “bankers” of the fens, and from the Irish peasants. The letter books and other records of the canal companies give immediate contact with these personal aspects of the subject, and Burton has constituted himself the “legger” making his way through them, not without profit. In spite of the recapitulation of much familiar material on other matters, these personal sidelights are what make the book worthwhile.

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J. MORDAUNT CROOK and M. H. PORT. *The History of the King's Works*. Volume 6, 1782–1851. London: H. M. Stationery Office. 1973. Pp. xxvi, 744. £12.50.

This remarkable project proceeds necessarily, and not inappropriately, at a slow and steady pace. Volumes 1 and 2, which deal with the Middle Ages, appeared in 1963, and although, eleven years later, those covering the Tudor, Stuart, and Hanoverian Works are well advanced, none are yet ready for publication. This final volume, published ahead of its chronological predecessors, is to be welcomed both as a visible—and reasonably priced—reminder of the impeccable standards of scholarship, format, and book production set by the first two

volumes (see *AHR*, 69 [1963]: 1117), and also because this account of the transformation of the Works, between 1782 and 1851, from a department of the royal household into a ministry under full parliamentary control goes beyond mere architectural history. It is also an important and hitherto neglected aspect of the overall growth of “government” in nineteenth-century England.

The first nine chapters are specifically concerned with changes in organization, personnel, and policy. These show clearly how, as Dr. Crook puts it, “the action of political ideas upon the structure of government is nicely reflected in the history of a single administrative unit” (p. 1). The Office of Works was reformed by the Whigs in 1782, the Tories in 1815, the Whigs in 1832 and again in 1851. In 1782, as a result of Economical Reform, all sinecures—here fully described and tabulated—were abolished and the office itself was absorbed into the reorganized royal household under the jurisdiction of the lord chamberlain. In 1815 the department gained its administrative autonomy, lost it in 1832 through the amalgamation with the Commission of Woods, Forests and Land Revenues, and finally regained it once more in 1851. Such changes, whether directed at an expanding royal household or at proliferating governmental bureaucracy—both traditional radical stamping grounds—clearly mirror current reform ideologies. They also reflect increasing Treasury initiative, whereby earlier, unsuccessful, attempts to control the expenditure of an incompetent Wyatt or a devious Nash gave way, especially after 1815, to a genuinely comprehensive control ranging from the initiation, scrutiny, and approval of all major projects to the provision in 1848 of carpets for the British minister's tents in Teheran! Small wonder that the same year Morpeth, as first commissioner, described the Works to the House as “a subordinate department of the Treasury” (p. 230). Several conclusions emerge from this account. One is that despite four reforms in eight decades, expenditures still increased. The detailed costs of each project over time are set out in a valuable appendix and expose the sheer irrelevance, in the face of spiraling costs of labor and materials, of both Whig reformist pretensions and the concept of centralized control. Furthermore, contemporary criticisms of departmental extravagance are not borne out by the evidence. The Office of Works habitually drove a very hard bargain with architects, contractors, and tradesmen. Ironically, as the Works under

Treasury surveillance became more committed to cost effectiveness, public response to such major projects as the new Houses of Parliament and the reshaping of Whitehall (the latter indeed the physical epitome of the growth of central government) was in terms of further Treasury controls. What also emerges is the strength of feeling—public, parliamentary, and within the civil service—against architectural *prima donnas*, whether “Attached Architects” like Nash (suspected rightly, of responding more swiftly to royal caprice than departmental directives) or commissioned outsiders like Barry. Brougham, for example, after three stifling nights of debate on the Maynooth grant in an unfinished, unventilated House of Commons chamber, described Barry’s assurance of completion as not “worth the paper on which it is written” (p. 225). Again it is paradoxical how, as the Works architects became increasingly professionalized and their role transformed from that of royal protégés to strictly accountable public servants, their professional integrity was increasingly impugned.

The remaining chapters, supported by excellent illustrations, provide definitive architectural histories of the increasing range of buildings for which the Works was directly responsible. These include royal palaces, public buildings, the new Houses of Parliament, prisons, and embassies. Several established judgments are overturned. Although the vexed question of Pugin’s precise contribution to the new Houses of Parliament is left unresolved, Port effectively disposes of Kenneth Clark’s contention that “every visible foot of the Houses of Parliament is the work of Pugin” (Clark, *The Gothic Revival* [1950], p. 175) and demonstrates convincingly that Barry’s control of the operation was somewhat firmer than many parliamentarians allowed. Likewise Smirke’s scheme for the British Museum was “almost certainly conceived”—*pace* Pevsner—“with no more than indirect reference to its Continental counterparts, Klenz’s Glyptothek in Munich and Schinkel’s Altes Museum in Berlin” (p. 403). There is, of course, much fascinating historical material embedded in the architectural narrative. We learn, for example, of Albert’s contention that at Buckingham Palace, with its faulty heating and inadequate kitchens, his queen was “worse off than any sovereign of Europe, the small German princes included” (p. 290), of the contractor’s refusal to endanger the lives of his men at the new Houses of Parliament by allowing them to work overtime by artificial gas light, and how in 1824 the locks at Kew Gar-

dens had to be changed to combat “marauders of botany” using counterfeit keys.

Above all, even those historians with no interest in either architecture or the nineteenth century will find the historical method employed here quite exemplary in the prudent hypothesizing, the patient accumulation of evidence, the closely wrought arguments expressed (especially when Dr. Crook is responsible) with clarity, incisiveness, and wit. The only regrettable omission is a bibliography, but in every sense there is much to be learned from this genuinely monumental work.

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ARNOLD THACKRAY. *John Dalton: Critical Assessments of His Life and Science*. (Harvard Monographs in the History of Science.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xiv, 190. \$11.50.

John Dalton’s career (1766–1844) embraced physics, meteorology, and chemistry. Dalton studies in recent years include the proceedings of a 1966 bicentenary conference, several biographical portraits, and an array of articles by historian of science Arnold Thackray. The trouble is that historians seldom read the lives of scientists except in the line of duty. Thackray’s volume of essays, based in large measure upon his material that has already appeared elsewhere, may not reform the reading habits of his colleagues, but just might persuade them that science is essentially a humanistic enterprise. John Dalton is remembered today because he breathed new life into the old Greek idea that matter is composed of atoms. While his scientific work is dominant in our memory, Thackray argues that other facets of his life merit equal attention to “help bring into focus the deeper challenge: an adequate analytical and interpretive account not just of one man or one region, but of science in the British Industrial Revolution.”

This book promises more than it delivers. Chapters 1 and 2 introduce the contents of the essays. Four of the eight chapters of the book follow traditional channels: chapter 5 traces the internal development of Dalton’s chemical atomic theory; chapter 6 consists of documents relating to the origins of the theory; chapter 7 is a selection of Dalton’s letters; and the final chapter is a bibliographic essay. But British science between 1760 and 1840 was not a monolithic enterprise. Dalton’s publication of *A New*

System of Chemical Philosophy in 1808 did not, for example, cast a spell on physicists and chemists alike. In fact, the study of the properties of matter in Dalton's century only became a shared enterprise in our own. Chapters 3 and 4, on the other hand, illustrate the trend among contemporary historians of science to study personalities and institutions of science within their cultural milieu. They treat in exquisite detail the social organization of science in Manchester—Dalton's home after 1792—during its industrial expansion. Topics covered include Dalton's early education, his close ties with other Quakers, his role in the newly born Manchester Literary and Philosophical Society, his means of support, and the changing historiography dealing with the internal development of the chemical atomic theory of matter. The rub is Manchester alone succeeded in carving out a scientific intellectual tradition of its own. Leeds, Liverpool, and Sheffield did not build up similar traditions.

Scientists search for patterns in nature; historians of science for patterns in the evolution of science. If Manchester's School of Science is part of a pattern, then it must be measured against its European and American counterparts in the nineteenth century. Thackray's "deeper challenge" needs redefining.

JUDITH R. GOODSTEIN

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GED MARTIN. *The Durham Report and British Policy: A Critical Essay*. (Cambridge Commonwealth Series.) New York: Cambridge University Press. 1972. Pp. viii, 120. \$9.75.

It is unfortunate that the Cambridge Press should have chosen Ged Martin's book as one of its Commonwealth series. A reassessment of the merits and defects of the Durham Report, and of Lord Durham himself as an imperial statesman, is long overdue. But Martin's thesis, which appears to be that they were of no value and had little or no influence in their own day, is carrying matters so far that one can only hope the little book will be discredited totally for most teachers of imperial history. Martin is perfectly accurate in saying that the now famous report was probably not read by the average educated Englishman, and that many of the political leaders of the day were so outraged by what they considered to be Durham's unpatriotic conduct in Canada that they had no desire to be guided by his advice. But Martin does not explain why in less than ten years

Nova Scotia and Canada had achieved self-government along the lines Durham had outlined. To say that the third Earl Grey and Lord Elgin adopted a policy all their own is absurd. Neither Grey nor Elgin ever denied their debt to Durham nor, for that matter, did Lord John Russell.

Lord Durham's policy was not put into effect, however, by Englishmen of either party but by Joseph Howe, Robert Baldwin, and L. H. Lafontaine on the other side of the Atlantic. In Upper Canada and in Nova Scotia the report was constantly quoted and with great effect. The issue on which the English ministers, and especially Peel and Stanley, would have preferred to hold out against the Durham influence was that of the control of the patronage, because they believed, and rightly, that the governor's real power must rest on his right to choose his own advisers. Immediately after the resignation of the first Baldwin-Lafontaine government—caused by the refusal of the governor, Metcalfe, to relinquish his control of appointments—Francis Hincks, the member of the cabinet who had the keenest sense for publicity, published as the leader in his new organ the *Pilot* that passage from the Durham Report in which Durham stated in no uncertain terms that if the colonies were to have self-government in the English sense of the words, the majority of the assembly must have control of the patronage. It was such passages much more than the more famous and general ones that were a guide and strengthening force to the reformers in Nova Scotia and Upper Canada.

Martin seems to have read no Canadian history published later than 1930. He has not listed in his bibliography those volumes in the Centenary History of Canada that deal with this period. Nor has he cited G. E. Wilson's *Life of Robert Baldwin* (1933) or J. M. S. Careless's *Brown of the Globe* (1959) in the early chapters of which are described the meetings of the Reform party with Baldwin, as chairman, quoting the Durham Report in almost every paragraph. Baldwin and Howe did not need Durham's statement of principles, which had probably originated with them in any case. But they did need the support of an English High Commissioner who never minced words or tried to hide his meaning. Their opponents in both colonies who had flourished under the old system, still flourished in the forties and put up strong resistance. Once they were conquered no one in England even attempted seriously to

oppose the complete triumph of "responsible government."

HELEN TAFT MANNING
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STEVEN MARCUS. *Engels, Manchester, and the Working Class*. New York: Random House. 1974. Pp. xiii, 271. \$8.95.

Most historians know Steven Marcus as the author of *The Other Victorians* (1966), a book noted for its unhistorical surprise that the sexual behavior of some Victorians did not live up to their supposed ideals. Marcus has the literary critic's vice, the exclusive reliance on an exiguous number of "texts," in that book "Pisanus Fraxi" and the notorious "Walter's" *My Secret Life*. But he also has the literary critic's virtue, a visceral sensitivity to the language, context, and undertones of his text, and in this "historical reclamation" of Engels's *The Condition of the Working Class in England in 1844* he brings to it a freshness and insight that go a long way toward rehabilitating Engels as one of the new race of men, in Tocqueville's phrase, who were giving new directions to nineteenth-century society.

Although Marcus uses W. O. Henderson and W. H. Chaloner's edition (1958) for reference throughout, he is extremely critical of their translation, and he pounces on the "mindless triviality" of their pseudo-Freudian explanation of the book as a thinly disguised attack on Engels's father. What Marcus sets out to do is to place the book in context, both as an astonishingly precocious peak in the career of a remarkable man and as a landmark in the development of modern industrial society as seen through its most typical domicile, Manchester, "the shock city of the age" as Asa Briggs called it.

On the one side, Marcus sees Engels not as a mere Oedipean rebel against his father but in a much subtler and more ambivalent relationship toward his family—one familiar to modern academics from the many middle-class radical students now or recently in universities—using the material and psychological security of a bourgeois home to criticize bourgeois society, "jumping into the abyss with a parachute." On the other, he shows how typical Engel's reactions were to Manchester by setting them beside those of Dickens, Chadwick, Faucher, and Tocqueville. Marcus's final conclusion is that Engels was both a revolutionary and a bourgeois, a man of the future and a rep-

resentative Victorian. To historians as opposed to literary critics this will come as no surprise.

HAROLD PERKIN
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DAVID LAYTON. *Science for the People: The Origins of the School Science Curriculum in England*. New York: Science History Publications. 1973. Pp. 226. \$10.00.

Teaching scientific ideas and the scientific method to the people has a social value; hence the title *Science for the People*. In his last chapter Professor Layton argues for the inclusion of science in the school curriculum, and his justification is no more convincing to the humanist than the insubstantial case made today for teaching history. The debate as to the proper balance between humanities and science in the school curriculum is an irresolvable point of advocacy without any sound basis in utility. One faction insists that students ought to know history, and the other side cannot see how a student in the modern world cannot know about science. Meanwhile, students drift to and fro on the tides of relevance.

The historical value of this book is that it is about a period in the history of English education when the problem was what to teach the children of the laboring poor. That question was, and is, I think, a real social question in that it transcends the vested interests of science and humanities teachers. Two factions introduced schemes of curriculum reform in different locations apparently unaware of each other until the 1850s when the debate was joined. One faction believed that the "science of common things" was proper for pupils. Starting with the familiar, students were instructed in the practical applications of a science such as chemistry to agriculture, diet, and hygiene. The curriculum was adaptable to rural and urban poor.

The other faction considered education in science as the best kind of education for all classes. Systematic, organized scientific thinking was good discipline for future physicians and lawyers. The intellectual level of the laboring poor was to be raised, not by teaching them industrial science or how to ventilate their homes better, but through the broadening educational experience of learning about scientific ideas and the scientific method.

Although the origins of the school science curriculum in England—the book's subtitle—are not the same as the American origins, the

issues are the same. By recounting the dispute over teaching pure science versus practical science to the laboring poor, Layton demonstrates that the issues were social more than intellectual. The outcome was determined by political power and social pressure. That is the way questions of educational policy were and are settled.

The social question of what to teach in the public schools—and public colleges as well—is inescapable. Those who make claims for the intellectual worth of their subject, whether the subject be physics or history, sooner or later will have to answer for the usefulness of their subject. Great educational reformers in the past have developed curriculums that met the demands of usefulness while giving due weight to the longer-lasting intellectual benefits of their disciplines. Layton has seen the history as one of a conflict in which one faction won. I think education in science as well as in history is one of a balance between opposing views, and the balance is a socially determined one.

HAROLD ISSADORE SHARLIN
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ROLAND MARX. *La Grande-Bretagne contemporaine, 1890-1973*. (Collection U.) Paris: Armand Colin. 1973. Pp. 318.

In the writing of national history it is often the foreigner who sees most clearly, particularly in explaining concepts that native historians too readily take for granted. One thinks immediately, for example, of R. K. Webb's account of a rather longer period of English history than Professor Roland Marx reviews here. Marx himself is a perceptive and sympathetic commentator, not quite an Halévy perhaps, but very strong in his grasp of the economic imperatives underlying recent British history. He is a complete master of all the recent research, and his judgments, if scarcely surprising, are extremely sensible. Marx believes that the issues which give the characteristic flavor to Britain's contemporary history were first clearly apparent around 1890 (a great improvement on those French historians who believe that all contemporary history began in 1789); but he gives due weight to the significance of the First World War. He calls the interwar period "the era of mediocrity," which suggests a rather old-fashioned, "pessimistic" treatment. But in fact Marx fully recognizes the substantial social developments of the period, and, in a valuable passage, favorably contrasts British economic development in the thirties with that of all the

other Western industrial countries. Most interesting, perhaps, is the treatment of the sixties and seventies where the loss of world status has pushed the United Kingdom on a different path toward integration with Europe. Given that there are now so many authoritative accounts of contemporary British history, it may well be that this excellent book will not be translated into English. But if it becomes the main medium through which French students are acquainted with contemporary Britain then one can feel very happy indeed about that.

ARTHUR MARWICK
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NICHOLAS D'OMBRAIN. *War Machinery and High Policy: Defence Administration in Peacetime Britain, 1902-1914*. (Oxford Historical Monographs.) New York: Oxford University Press. 1973. Pp. xvii, 302, 1 table. \$14.50.

This is a thought-provoking book based on the archives of the Committee of Imperial Defence (CID), the War Office, and the Admiralty and an impressive array of private sources. In describing the relations between the CID and the service departments, d'Ombraïn successfully challenges the view of the committee as a center for strategic planning and a coordinator of preparations for war, and he explains the reasons for the gap between strategy and capability with which Britain went to war in 1914. The early CID was a quasi-executive body based on a maritime and imperial strategy in which the "senior service," re-equipped and revitalized by Fisher, was to play the leading role. The army, its weaknesses revealed during the Boer War, accepted its secondary position until the first Moroccan crisis provided it with a new *raison d'être*. The men at the War Office, particularly Henry Wilson, became committed to the *military entente* (author's italics), an expeditionary force, and the French doctrine of the offensive. Given the political impracticality of conscription, this continental commitment was assumed without any provision for the necessary mass army. The CID and the Admiralty both opposed this new orientation, but the former was unable to prevail without bringing its very existence into question and the Admiralty, recognizing its impotence within the CID gradually withdrew its support. After 1909 the CID ceased to be an executive body determining national policy. Its work was done in a large number of subcommittees dealing with the technical problems of defense planning. This concentration on administrative minutiae prolonged the existence of the committee but dis-

guised the dislocation between policy and the physical capability of the two services. The army and navy had two different strategies; it was the former that gained in prestige and importance but the navy which had the resources.

D'Ombra's strong views give considerable life to this closely argued monograph. Despite the author's sympathy for the Admiralty position, one wonders whether the navy—or the cabinet—understood the changed nature of Britain's diplomatic position or the kind of war she would fight. Though Fisher argued that blockade rather than invasion was his main concern, the navy concentrated on its defensive role, on capital ships and not on submarines or mines. The author stresses the continental commitment; he says little about the naval agreements of 1912 (see Paul Halpern, *The Mediterranean Naval Situation 1909–1912* [1971]). He suggests that the Foreign Office was committed to the *military entente*—the words do not appear in the Foreign Office records—but the diplomatic evidence suggests that Grey never fully understood the political implications of the military conversations and was reluctant to send an expeditionary force to France in August 1914. The extent to which the diplomats had lost their freedom of action because of the military and naval ties with France remains a subject of debate, and this provocative book should find an audience among diplomatic as well as military historians.

ZARA STEINER
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R. F. MACKAY. *Fisher of Kilverstone*. New York: Oxford University Press. 1973. Pp. xvi, 539. \$19.25.

Few figures in the long history of the Royal Navy are more colorful and important than John Arbuthnot Fisher, and he has already received a generous measure of attention from historians. The first question that must therefore be asked about any new study of Fisher is whether it adds anything to what we already know from other works, such as those of Arthur Marder. The answer in this case is that it certainly does. Mackay has devoted half of this substantial and well-written study to Fisher's early years and has been able to draw upon material that was not available to Marder. What emerges clearly is the early development of traits that were to mark Fisher during his years of power: his concentration on questions of matériel was, as Mackay points out, in-

evitable in an era when the navy was struggling to adapt itself to the ever-accelerating pace of technological change. The lack of strategic or tactical clarity in Fisher's thought stemmed from the same source—it was hard to know what the fleet could do in war if its nature and weaponry were constantly changing. The less attractive side of Fisher was likewise evident very early: the sudden veering from friendship to bitter enmity, the taste for intrigue, and the careful cultivation and manipulation of friendly journalists. Mackay gives a fair and balanced account both of Fisher's reforms and of the famous feud with Lord Charles Beresford. But perhaps the most interesting section of the book is Mackay's analysis of the Fisher-Churchill relationship. He argues that Fisher did not really believe in the Baltic operations about which he talked and wrote so much. These were simply ways of beating off pressure for amphibious operations against the German North Sea coast or the Dardanelles, in which he did not believe either, for he had come to the conclusion that in the prevailing conditions only distant blockade was possible. The belief that Fisher wanted a great Baltic operation is a product, Mackay claims, of the testimony, concerted beforehand with Churchill, given to the Dardanelles Commission and later echoed in Churchill's *World Crisis*. It is an interesting, if not completely convincing, thesis. In particular it does not explain some of the aberrations in Fisher's behavior before and during the crisis of May 1915. Nevertheless, Mackay has made an important contribution to the history of the "Fisher era," and one that no historian interested in the period should neglect.

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JEAN VAN DER POEL, editor. *Selections from the Smuts Papers*. Volume 5, *September 1919–November 1934*; volume 6, *December 1934–August 1945*; volume 7, *August 1945–October 1950*; *Biographical Notes and Index*. New York: Cambridge University Press. 1973. Pp. vii, 621; 551; 496. \$47.50 each.

These three superbly edited books, the second part of the seven volumes of the *Selections*, mark the end of a great historical enterprise, completed in four stages: the Smuts papers housed as the Smuts Collection in the state archives in Pretoria, the two volumes of the biography by Sir Keith Hancock, and the two sections of the *Selections*. Leaving only three volumes for the last thirty years was an un-

fortunate decision. Biographical notes and the analytic index—a masterpiece—needed 127 pages. Completely or virtually eliminated were incoming letters, academic writings, and nearly all official documents and key speeches. The effect is to focus attention on Smuts's own letters. The frequency of the letters and the editorial notes preserve continuity. Smuts's letters to his Quaker friends in Oxford are the core of the volumes. Knowing they were preserved and closely guarded he could treat them as a safe deposit for recording his activities and many interests, even the probings of his deeper self. In the privacy of the letters his mind could play like summer lightning on the riddles of man and the cosmos, always reaching for the unities behind the chaos of particulars—themes of his book on holism and evolution. Sometimes he wondered whether his preoccupation with unity might not be linked with some sense of inner disharmony beyond his conscious reach. "I am a wanderer," he wrote, "seeking if haply I may find what it is for no man to find." His biographer asked, "General, what is the secret of your being?" He replied, "My secret is that I am a very uncoordinated man." Yet it was the coordination in a rare degree of great faculties that was one of the secrets of his success. No one in any part of the Commonwealth received such spontaneous recognition for achievement at such a high level in so many fields. The long list included the rare Order of Merit—limited to eight members—and field marshal of the British army.

For thirty years he was a commanding figure in South Africa, continuously prime minister or leader of the Opposition. He had a global interest in foreign affairs. Causes especially dear to him included Israel, the League of Nations, and the Commonwealth. A memorandum he wrote in 1921 on the Constitution of the British Commonwealth is singled out by the editor as of "primary importance." It was comparable in importance to his famous brochure published in December 1918 laying the foundations for the Covenant. His memorandum sought to do the same for the Commonwealth. It was never circulated, despite a prefatory editorial note to the contrary. Even in official circles its nature and content remained unknown. Hughes scuttled it in 1921 before it could be launched. Hertzog succeeded in 1926 in sinking it without trace. He found the only copy among Smuts's papers, took it over, and kept it secret. By reading part of one long paragraph—of minor importance without the context—Hertzog left the prime ministers with the im-

pression that this was all Smuts had written. In this passage Smuts referred to my proposal, published in 1920, to resolve the dilemma of the Commonwealth by a "general declaration of constitutional right." "I heartily endorse Hall's suggestion," Smuts concluded, "which seems to me the easiest constitutional means of settling the international status of the Dominions without changing the unwritten flexible character of the British Commonwealth." The memorandum had shrunk to a few lines and was forgotten until 1953 when I discovered a copy in the Confidential Print of the Colonial Office. A search in the Smuts Collection found nothing.

The war in 1939—the "Great Horror" Smuts had sensed dimly in a letter in 1919—thrust him once more on the "dreadful treadmill of duty." Prime minister, head of three departments, commander in chief, rebuilder of the armed forces equipping them with arms manufactured in the Union—there seemed no end to the duties.

At the end he led the Union's delegation at San Francisco. The preamble to the Charter, which he drafted, expressed his hopes but not his forebodings. When the General Assembly met in 1946 his hope of concentrating on "the immense issues of our human future" were dashed. He found his country and himself pilloried and made the world's scapegoat for racialism, with India "the greatest country of discrimination and communal disunity in the world" leading the attackers. Still pondering months later on the meaning of it all, Smuts wrote, "Some vast change is silently coming over the course of history. . . . The U.N.O. gave me my first great knock." The second was his defeat in the election of 1948 by a less liberal party. Duty still called him to lead the opposition until the end in 1950; but he did not cease from hoping. Almost his last words were, "The world's great age may be waiting round the corner."

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MAURICE R. O'CONNELL, editor. *The Correspondence of Daniel O'Connell*. Volume 1, 1792–1814; volume 2, 1815–1823. New York: Barnes and Noble, for the Irish Manuscripts Commission. 1973. Pp. xxxi, 396; ix, 543. \$22.50; \$23.50.

Daniel O'Connell deserves the designation, "greatest of all Irish nationalists." He created and shaped the character of Irish nationalism, endowing it with liberal democratic principles.

O'Connell's influence extended beyond Ireland. He won victories for radical causes in the oppressive Age of Metternich, providing tactics for and inspiring the forces of the Left in Britain and on the Continent. Despite his significance, there is no satisfactory scholarly biography of him. The lack of research materials, particularly private papers, has contributed to this omission. W. J. Fitzpatrick's *Correspondence of Daniel O'Connell* (1888) has been a useful but inadequate source. In an effort to establish a firm foundation for O'Connell studies, Maurice O'Connell, assisted by his wife Elizabeth M. O'Connell, Gerald J. Lyne, and Hugh McFadden, has collected and organized material from a wide variety of sources and places for a projected eight-volume series of O'Connell letters.

Volume 1 of *The Correspondence of Daniel O'Connell* covers O'Connell's secondary-school days at St. Omer and Douai and in London; legal studies in London and Dublin; admission to the Irish Bar; marriage and role as husband and father in a rapidly expanding family; increasingly prosperous legal practice; complicated relations with Kerry relatives; and involvement in and leadership over the Catholic civil rights movement. Volume 2 surveys O'Connell's refusal to accept British government control over the appointment of Catholic bishops (the Veto) in exchange for Emancipation and the consequent split in Catholic ranks, extreme financial difficulties at a time when his professional income was high, and the beginnings of a new Catholic Association that would eventually bring victory to the Catholic civil rights effort.

Letters to and from O'Connell indicate that contrary to myths and enemy accusations he was a devoted and loving husband. He was also an affectionate and proud father, totally reckless and incompetent in money matters, a warm friend but not always wise in the selection of associates, a benevolent landlord for the times, an optimist, professionally ambitious, and a genius in matters of political organization, but frequently naive in evaluating political situations.

O'Connell was passionate in his commitment to liberty, but he seldom tried to intellectualize his radicalism in letters. He supported parliamentary reform in the United Kingdom, sympathized with rebels against the Old Regime in Spain and Portugal, furnished a son to Bolívar's liberation movement in Latin America, and often expressed contempt for the Bourbons and admiration for Napoleon as an agent of

Enlightenment and freedom. The letters trace O'Connell's passage from deism to practicing to pious Catholicism. In his last condition he re-evaluated Bonaparte but did not reject liberal values. He never bothered to come to terms with the paradox of his loyalty to the Left and his devotion to a politically reactionary Church. While advocating Irish freedom, O'Connell admired British constitutional principles and practices and became a Francophobe.

Much to the anger of physical-force nationalists, O'Connell consistently opposed revolution—he thought Emmet deserved execution—but his animosity to violent freedom movements in Ireland was more tactics than pacifism. In 1798 he enlisted in a lawyer's yeoman corps, and in the early 1820s he was anxious to soldier against Whiteboys and Orangemen.

O'Connell's correspondence reveals the grim environment of early nineteenth-century Ireland: secret-society violence, famine and peasant poverty, Orange oppression, Protestant Ascendancy, and conflicts among Catholic leaders. Some wanted status within the British establishment; others, like O'Connell, wanted more, the freedom and welfare of their people and nation. Helen Mulvey introduces volume 1 with a justifiably sympathetic portrait of Mary O'Connell and her marriage with Daniel. Although these two volumes could use more explanatory footnotes, Maurice O'Connell and his associates have presented a superb example of collecting, selecting, organizing, editing, and footnoting the correspondence of an important and interesting man. These volumes are a significant addition to Irish historiography, and they whet the appetite for the feast yet to come.

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GERVAIS E. REED. *Claude Barbin: Libraire de Paris sous le règne de Louis XIV.* (Centre de Recherches d'Histoire et de Philologie de l'École Pratique des Hautes Études de la IV^e Section. Histoire et civilisation du livre, 5.) Geneva: Librairie Droz. 1974. Pp. 131.

Collecting information dispersed in scattered documents, some difficult of access, Dr. Reed offers in this slender volume the first reliable account of the career of Claude Barbin (1624–98), Parisian publisher of writings from all but a few of the French classical authors. But sparseness of information and brevity of commentary give Reed's contribution more the

character of an excellent and extended encyclopedia entry than a full-scale biography. Since membership in the Communauté des Maîtres Libraires et Imprimeurs de Paris was extremely limited, and since documentation about one figure often communicates knowledge about others, we can only wonder why Reed did not go on to investigate characteristics common to the entire group. A little more labor could have made all the difference between communicating some useful biographical information and writing a prosopographical essay of enduring value.

But however limited it is in scope, scholars will profit from Reed's book, especially the indexes, which include *inter alia* the thirteen-year-old Barbin's *brevet d'apprentissage* and a complete short-title list of Barbin's yearly printings with a library call number for each entry. Reed adds, moreover, a few new details to the pioneering and fundamental work of Henri-Jean Martin (*Livres, pouvoirs et société à Paris au XVII^e siècle, 1598-1701* [Paris, 1969]). One hopes that we can now look forward to more ambitious scholarship which will paint a clearer picture of the Communauté des Maîtres Libraires during the *ancien régime*.

LIONEL ROTHKRUG
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JEAN ORIEUX. *Talleyrand: The Art of Survival*. Translated from the French by PATRICIA WOLF. New York: Alfred A. Knopf. 1974. Pp. xii, 677, xxiii. \$12.95.

This tendentious book claims to remove the masks from an intriguing figure of French history. Published originally as *Talleyrand ou le Sphinx incompris* (1970), it represents another *douceur* collected by Talleyrand from those who wish to traffic with him. As in his life, the market place often saves him from the penury he most feared, the state of being neglected.

Jean Orieux, prize-winning novelist and biographer, argues that "the contentious, anarchistic climate of our times" (p. x), rather than the works of previous historians, now allows us to lift the masks from the sphinx who will show us his true face if we do not lecture him. One has doubts. Judgments on Talleyrand will continue to vary depending upon perspectives. Would he be displeased?

Orieux sees his hero as a kindly man loyal to friends, family, and subordinates. Talleyrand personifies the humane and elegant image of what many imagine the eighteenth century

to have been. The peculation, the ingratitude toward Madame de Staël, the initial use of a *Senatus consultum*, and the Enghien affair are all mentioned and not defended. But overall, Orieux eulogizes Talleyrand as a sensitive man who always served France while serving himself.

Historians will be disappointed with this book. The thesis hardly differs from Crane Brinton's. Lacour-Gayet's volumes, challenged implicitly, are at times closely mined for coverage and quotations (compare pp. 614-29 with Lacour-Gayet, vol. 3, ch. 24). In this instance the best that can be said is that Lacour-Gayet said it before. Historical errors not in the original creep into Patricia Wolf's translation: the clergy own most of the arable land in 1789 (p. 77); the Committee of Public Safety is still in existence on 18 *fructidor* V (p. 179); Napoleon committed France at the peace of Lunéville to pay her entire debt to Spain (p. 278); at Tilset Alexander I was promised the remains of the Ottoman Empire plus Moldavia and Wallachia (p. 339).

Although the book does not contribute to our knowledge of Talleyrand, it is a readable survey useful for the general reader for whom it was designed. It depicts the life of a man who showed, as Orieux contends, a self-mastery and an ability to shield his personality through times where many were more disfigured. Perhaps he did represent the possibilities of the *ancien régime*; Orieux would prefer to say Voltarian civilization. Still, one sheds no tears for Talleyrand as one might for Rousseau or Robespierre. He better than they could bear the reproaches of contemporaries and future generations. He would not have been chagrined at the allusions to masks and to the sphinx. As he enjoyed life he must, if he is able, still savor the conversation.

ROBERT C. ADAMS
University of Tennessee,
Knoxville

ADELINE DAUMARD *et al.*, editors. *Les fortunes françaises au XIX^e siècle: Enquête sur la répartition et la composition des capitaux privés à Paris, Lyon, Lille, Bordeaux et Toulouse d'après l'enregistrement des déclarations de succession*. (École Pratique des Hautes Études-Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisation et sociétés, 27.) Paris: Mouton. 1973. Pp. xv, 603.

This collaborative work, employing inheritance tax records (*déclarations de mutations par*

décès), provides new and significant information on the distribution and composition of wealth in urban France. This is the most ambitious work using inheritance tax records yet to appear, covering five of the largest cities: Paris, Lille, Bordeaux, Toulouse, and Lyon. The records of three or four complete years between 1820 and 1911 were utilized for each city. Since the contributors employed the same method and covered approximately the same years, it is possible to compare the evolution in the distribution and composition of wealth in these cities from the Restoration until the eve of World War I. They have also analyzed the changing distribution and composition of wealth among socioprofessional categories.

The book is divided into two parts. The first part, written by Adeline Daumard, discusses the potentialities and limitations of inheritance tax records as a source for social and economic history. These records are comprehensive, for detailed declarations were required, until 1901, from all who were not completely destitute. Fraudulent declarations are a problem, but apparently not as important as one might suppose, since concealing or undervaluing most assets was difficult. In some cases, the heirs were interested in accurate evaluations. Daumard also provides future researchers with a model for the reconstitution of individual fortunes. And, most important, she combines the data of the five studies to provide an overall synthesis. The second part of the book, comprising over four hundred pages and including more than one hundred tables of statistics, contains individual monographs by Adeline Daumard on Paris, by Félix Codaccioni on Lille, by Georges Dupoux and Jacqueline Herpin on Bordeaux, and by Jacques Godechot and Jean Sentou on Toulouse. Although the results of Pierre Léon's study of Lyon are incorporated into Daumard's synthesis, it will be published separately.

Some of the conclusions are not surprising: during the nineteenth century the relative importance of landed wealth declined, that of negotiable securities greatly increased. What this study provides is the exact magnitude of these changes. Other conclusions are less obvious: in four of the five cities, Toulouse is the exception, over two-thirds of all deceased adults had nothing to leave behind during the Restoration, and almost a century later the proportion remained the same. In addition, small estates valued at less than five hundred francs were numerous. Whatever may have been the case in the countryside and in small

towns, a hundred years of economic progress had not narrowed the inequalities of wealth in large urban centers. That is only a brief indication of what is to be found in this important work.

CHARLES E. FREEDEMAN
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Binghamton

ETIENNE VAN DE WALLE. *The Female Population of France in the Nineteenth Century: A Reconstruction of 82 Départements*. (A publication of the Office of Population Research, Princeton University.) Princeton: Princeton University Press, 1974. Pp. xix, 483. \$21.50.

France is used as a predictor or paradigm for European fertility decline. Decline is observable in France before 1830; elsewhere it comes after 1875. Biological characteristics make females the basic unit for fertility studies. But as Professor van de Walle shows in a good brief discussion, French census data are biased and incomplete—a helpful caution for those using or evaluating work using this material uncritically.

To correct this the author “reconstructs” the female population. It is a method of “simulating populations from aggregate vital registration data” (p. x) and “from the coherence of the whole . . . assess[ing] the reliability of the details” (p. 57). Internal adjustments and controls—for example, migration and mortality—are made to improve the fit of the parts and create satisfactory, unbiased results. Self-critically and patiently the author guides the reader through the technique and notes the special characteristics and biases of the reconstructed and official statistics.

Data are given by *département* for total female population, birth distribution, marriage by age, and overall, illegitimate, and married fertility rates. Because of administrative changes several border *départements* are omitted. Heavy in-migration made others—Seine, Seine-et-Oise, Rhône, Bouches-du-Rhône—“intractable” to reconstruction.

With different criteria Jean Bourgeois-Pichat reconstructed the total population of France. Van de Walle's technique and results for females are more convincing. Could reconstruction be “adapted to men with a few changes” (p. viii)? Given the vaster male migration, with its geographic, age, and occupational variations, it is a conjectural claim. A steady-stream pattern of statistical control is inadequate. Nominal, individual data from other sources

(a task for regional historians especially) are needed to judge the reconstruction more accurately.

The author plans another volume using these statistics to explain the reasons for fertility decline. But data by *départements* present a problem. *Départements* can be valued economically, culturally, and politically, but is social differentiation measured within geographic units? The influence of class on fertility cannot be analyzed from departmental statistics; nominal research can add this dimension.

Historians interested in these eighty-two *départements* or females must consult this work. For French historians it underlines the need to develop data to supplant biased official results. This clear presentation of reconstruction gives one technique; one hopes that it will suggest others to deal with similar problems. The methodological and factual worth of this volume promises to make the second of interest to all European historians.

MARTIN R. WALDMAN

City College,

City University of New York

DOUGLAS PORCH. *Army and Revolution: France 1815-1848*. Boston: Routledge and Kegan Paul. 1974. Pp. x, 182. \$13.25.

Readers in quest of a book complementary to those of Vidalenc, Chalmin, or La Gorce as an investigation of the social and political history of the French army under the *régime censitaire* will not be satisfied by Porch's monograph. Despite its title, its chronological focus is on the last seven years of the Restoration and the first six of the July Monarchy. Only a single page is devoted to the period after the abortive Strasbourg coup of 1836. The book is organized as a series of relatively discrete chapters on such limited topics as the recruitment law sponsored by Marshal Soult, republicanism in the artillery, and a military "rebellion" at Lunéville in 1834.

Its thesis is never explicitly stated as such, but the author repeatedly asserts that revolutionary dissidence by soldiers was far less a result of partisan ideology than it was of professional dissatisfaction. To support this contention, his exhaustive chronicle of suspected and actual military subversive activities during these thirteen years is interspersed not only with familiar political narrative (which situates the well-known massacre of 1834 in the "rue Transmonian") but also with nineteen statistical tables and six appendixes. These are in-

tended to enumerate such presumable causative factors of political behavior as the geographical and social backgrounds and the career patterns of suspect soldiers. Unfortunately many of these statistics are accompanied by a cautionary footnote that they can be "regarded only as samples," but nowhere is it explained whence any of these relatively small "samples" have been taken, how representative they are, and how they might compare with a general statistical profile of each entire category of the French army in question. (The author has used some unpublished material from Vincennes, but in the absence of a critical bibliographical essay the reader cannot really estimate its scope or its utility either to Porch or to other historians.)

It cannot be said that these fragmentary figures unambiguously substantiate Porch's implicit thesis. For example, almost one-third of his sample of infantrymen arrested for Republican activity under Louis-Philippe had been recruited from the departments of the Seine and the Rhône, that is, from those regions of France where Republican sentiment was probably most deeply rooted in the population. No doubt the insufficiency of pay, pensions, and living conditions helped to fan barracks discontent into political disloyalty, but it is difficult to follow Porch's contention that the alleviation of some of these conditions in 1834 was essential in transforming the army into the "great mute," since by his account similar improvements instituted in 1831 had not ended opposition in the ranks. Nor is it necessarily obvious that "if opposition activity in the army had been politically inspired, it would have been carried out by young men who joined . . . immediately after the 1830 revolution." The political behavior of French soldiers in the 1830s, like that of most people, undoubtedly was inspired by conscious attitudes and specific events as well as by the conditions of their lives, which soldiers in any case could quite justifiably conceive as being determined by the values and spending priorities of the successive regimes they served.

JOHN ROTHNEY

Ohio State University

LEO A. LOUBÈRE. *Radicalism in Mediterranean France: Its Rise and Decline, 1848-1914*. Albany: State University of New York Press. 1974. Pp. xvii, 258. \$20.00.

This book has been long in the making. It was conceived in studies of Left radicalism in the Third Republic that drew attention to the

Midi. Articles followed on Meridional radicalism, which developed the thesis of this volume. There was an excursion into the mining areas of northern Languedoc. Now all is put together and extended into Provence.

Topography, climate, and history have given a certain unity to this Mediterranean littoral: Professor Loubère adds a more recent unifying factor—commercial wine. Viticulture has often produced a conservative mentality; in the Midi it acted as a magnet to draw population to the semiurban coastal plains where winegrowers became socially mobile, receptive to new ideas, and expectant of government aid. Economic interest, a social structure favorable to change, and cultural variables must all be invoked to interpret Mediterranean radicalism.

Three major stages receive explanation: the strength of the Left during the Second Republic and its resistance to the coup of 1851, which left an enduring tradition; the Radical dominance built during the 1880s partly in reaction to the devastation of phylloxera; the period of the decline of southern radicalism, generally to the advantage of socialism, in the early decades of this century at a time when the Radicals were becoming dominant in much of France.

Variables affected the pattern. They could result from policy: Radicals did not win a following beyond the wine area until they became protectionist; from class response, for although workers were always important, in the Var, Radicals were strongest in owner-dominated areas; from religion in radicalism's attraction to Protestants in the Gard; from leadership or Masonic connections; even from sentiment in the appeal to the memory of the Albigenians who were also victims of the North. All these are woven in, though always the stain of the grape is apparent in the final pattern.

For a regional history to be classed as excellent it should deal with an interesting area that has problems of wider significance, and its painstaking research should be objectively evaluated. This volume qualifies on each count.

JOSEPH N. MOODY
Catholic University of America

ROBERT R. LOCKE. *French Legitimists and the Politics of Moral Order in the Early Third Republic*. Princeton: Princeton University Press. 1974. Pp. x, 321. \$14.50.

The title of this revised dissertation (1965) should not mislead; Locke leaves political nar-

rative of the republics of Thiers and the Dukes to Hanotaux and Halévy. Locke's concern is with legitimism, "considered against the long-term process of social, economic, and political change in nineteenth-century France," "a socioeconomic study of political psychology." He selects as specimens 201 deputies of 1871, mostly Gouault's "Right" and "Extreme Right," with some unexplained additions. Methodological purists will experience, as they usually do, some discomfort over extrapolating from so few "ists" to an "ism," but no matter; Locke qualifies his judgments intelligently and points the way to further studies.

The apparition of a host of legitimists in the National Assembly has seldom failed to provoke expressions of wonderment tinged with derision. Locke turned his wonder to inquiry. He mainly confirms in a specific context features or tendencies of legitimism noted elsewhere—Tudesq, Rémond, and others. The special interest of the book lies in the picture of the deputies themselves, particularly as drawn from materials obtained at great personal effort from hitherto unopened family archives and, for the elections of 1871 and 1876, an unprecedented scouring of the provincial press. Locke tells us who these men were (for example, forty-five per cent were *not* nobles); how they got elected; what they thought, intended, and settled for doing; what distinguished them from Orleanists; and above all whether they truly were anachronisms, survivors of socioeconomic elements doomed by advancing industrialism and centralizing government. A concluding chapter thankfully helps to disentangle some complex argumentation, especially on the latter point; the author's expositions too frequently do injustice to his research and insights.

A coupling of landed nobles and representatives of old Catholic urban oligarchies, the legitimists nevertheless had quietly renounced desires to bring back the society of the Old Regime. They were willing to come to terms even with an industrial revolution, but not with the liberal ideology conventionally assumed to be necessary to the great transformation. (Their Prussian and Japanese counterparts had no revolution to contend with.) The concerns they expressed for family and community and their rejection of rampant individualism and prideful reason echo strangely a century later. There were anachronisms, but perhaps not precisely in the way we thought.

DAVID S. NEWHALL
Centre College

LADISLAS MYSYROWICZ. *Autopsie d'une défaite: Origines de l'effondrement militaire français de 1940.* (Collection Historica.) Lausanne: L'Age d'Homme; distrib. by International Scholarly Book Services, Portland, Ore. 1973. Pp. 385. \$9.25.

Although it is concerned with some of the fundamental reasons for the collapse of the French army in 1940, this book is more an essay in intellectual history than it is military history in the usually accepted sense. The aim of the author is not so much to demonstrate how the French army, relying on a defensive strategy, was defeated by the German army but rather to elucidate the assumptions held by the military which underlay that strategy.

French theorists, having recognized the pitfalls of the overly abstract doctrine disseminated before 1914, turned to an empirical and positivist approach to war. They refused to stray beyond what were taken to be the explicit lessons of recent history concerning the absolute ascendancy of material factors in combat, and in so doing they "exalted to the level of immutable principles the transitory conditions of 1914-1918." The result was a prudent but rigid doctrine of war that, among other things, confirmed the pre-eminent role of the infantry. Battle was conceived to be a methodical, predictable affair, and its constituent elements measurable with mathematical precision. It was in terms of this doctrine that French military authorities apprehended the tank and the airplane. Armor was incorporated into the existing military system, but to the detriment of its primary attribute, mobility. Since there was no possibility of fitting the airplane into current doctrine without in effect undermining it, no one objected to a divorce between the ground and the air forces and the organization of the latter under a separate ministry.

At no point in his study does the author argue that there was any alternative strategic doctrine available for France, given the presuppositions of not only the professional soldiers, but also the politicians and the French people. He rejects the one advocated by Lieutenant Colonel de Gaulle as not being militarily realistic, at least not in the mid-1930s, when it was publicized and discussed. By way of an attempt to explain this failure of intellect and imagination, Mysyrowicz depicts somewhat impressionistically the desperate revulsion toward war felt by French society after 1918, suggesting that "a doctrine of war always incorporates . . . the spirit of its time."

In his account of the origins and evolution of French strategy in the interwar years, Mysyrowicz does not arrive at any novel conclusions. The considerable merit of this work lies rather in his perceptive re-creation of the mental outlook of the professional soldiers and in his trenchant analysis of the postulates, intellectual and institutional, of French military doctrine.

DAVID B. RALSTON

Massachusetts Institute of Technology

J. L. PRICE. *Culture and Society in the Dutch Republic during the 17th Century.* New York: Charles Scribner's Sons. 1974. Pp. 260. \$12.50.

This book attempts to explain why the seventeenth-century Dutch were able to produce an outstanding and independent school of painting but were unable to produce a similarly distinguished and independent literature. In one sense the author delivers more than he promised. As background he treats in satisfying detail and clarity the complex politics of the republic, the tensions between Orangists and the States party, the relationship between Holland and the other provinces, church-state affairs, the primacy of the towns, and the character of the regent class. While not the result of new research, these chapters provide a brief and accurate introduction to seventeenth-century Dutch society.

Price contends that the literature of the republic was largely produced for and by the loosely defined regent class of its richest province, Holland. These wealthy and better-educated people imposed their Renaissance-classical tastes on Dutch literature. They might have done the same in art, but most Dutch painters were artisans unfamiliar with Renaissance theories or classical themes. They catered to the large popular market for art as part of home decoration. Prices were low, the artists' expectations were modest, but the volume of painting was very large. Consequently, Price concludes, Dutch artists remained independent of the general modes of seventeenth-century European art. The chapter on painting is the best in the book.

Price does not discuss the Dutch overseas empire and its effects on Dutch culture. He dismisses the East Indian trade as a minor fraction of Dutch commerce and asserts that its cultural effects were minimal, although he admits that contemporaries were probably more impressed with the East Indian adventure than with any other aspect of Dutch culture (pp. 53-55).

He says that Holland was Europe's major center of book publication but does not mention the huge volume of travel literature that poured from Holland's presses and helped maintain their pre-eminence. Nor does he discuss the large place occupied by the travel literature in Dutch popular literature—literature probably read by the same people who bought the paintings. Its influence can also be seen in the works of some of the major poets and playwrights. In fact the influence of the overseas contacts can be found in many aspects of Dutch culture, and its total omission to some extent impairs what is otherwise a very useful introduction to the seventeenth-century Dutch republic.

EDWIN J. VAN KLEY
Calvin College

J. J. POELHEKKE. *Geen blijder maer in tachtigh jaer: Verspreide studiën over de crisisperiode 1648–1651* [No News Pleased More in Years Four Score: Scattered Studies of the Period of Crisis 1648–1651]. Zutphen: De Walburg Pers. 1973. Pp. 270. 28.50 gls.

The rhyming main title of this book is taken from a famous quatrain deposited, with a multiplied contribution, in an Amsterdam church after the death of Prince William II of Orange in early November 1650—a demise that marked the beginning of the last act of a drama which began in January 1648 with the signature of the treaty of peace with Spain at Münster. The eight articles in this book, collected from well-known and obscure journals, are devoted to this crisis in two of its aspects: first, the political struggle between the Holland “empocracy”—the author’s pseudo-Aristotelian term for the merchants’ rule—and the House of Orange and its allies, especially in Gelderland; second, the failure of Dutch Catholics to achieve a formal improvement in their status beyond quasi-legal tolerance even after the end of the war with the *Rey Catolico*. The articles are masterpieces in the exploration of significant nuance. Poelhekke seeks out large meanings in small things, such as pamphlets, a minor riot, an obscure diplomatic episode, and the politics of a poor province and one of its towns (the Nijmegen where he is now professor of history at the Catholic University). Dissecting and analyzing with great care and subtlety, with an erudition that humor and style save from any touch of pedantry, the author draws out the conclusion that William II, like his father, uncle, and grandfather, was deeply committed to a “Great Netherlands” policy, but one that

was based on the historic Burgundian state rather than the strictly Dutch-Flemish-speaking territory favored by the late Pieter Geyl. The implications of these articles will echo long, even if softly and slowly, in thoughtful historical work on the Dutch “Golden Age.” The knowledge, understanding, and expressive power they display make one impatient for Poelhekke’s biography of Prince Frederick Henry, at which he is still at work.

HERBERT H. ROWEN
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New Brunswick

BRYCE LYON. *Henri Pirenne: A Biographical and Intellectual Study*. Preface by F. L. GANSHOF. Ghent: E. Story—Scientia; distrib. by Humanities Press, Atlantic Highlands, N.J. 1974. Pp. xix, 477. Cloth \$19.00, paper \$15.00.

The dwindling number of admirers who knew Henri Pirenne personally will cherish this fine appraisal of the man and scholar they revered; others will now be able to understand better the esteem he enjoyed during his long and brilliant career. The book is a model of what such a work should be, and it holds attraction for any cultivated reader. It will inspire in historians greater respect for their craft and dictate for them what can be achieved by hard work, a highly trained mind, and supreme talent. Bryce Lyon, with the assistance of his wife Mary, has placed us all in his debt. He never knew Pirenne, but with great learning and sensitivity Lyon has been able to write a story that will need little if any correction. This is no mean accomplishment, nor was it an easy task to perform. Fortunately, his sources proved to be far greater than was originally suspected. Pirenne was a continuous and systematic collector, a hoarder it would seem, of notes, manuscripts, and mementos, a writer of thousands of letters, and a keeper of several diaries. These and the writings of his many friends, as well as his own scholarly contributions, have been carefully explored. Happily also, many who knew Pirenne aided the author. First among these was Count Jacques Pirenne who placed at Lyon’s disposal the resources of the Pirenne family collections, as well as supplying priceless recollections of his distinguished father.

As François Ganshof remarks in his perceptive prefatory note, this book is the story of a personality, that of the greatest medievalist of the twentieth century. With these assertions few would disagree, for they are indeed the essence of Pirenne’s stature. It is a fascinating

experience to follow this story of the bright lad from the upper-middle-class family in Verviers from his birth in 1862 to his untimely death in 1935. Family business investments provided ample funds for broad schooling, and fortunately within his circle culture was respected. It was, however, only with perseverance and tact that the young Henri was able to avoid family pressures to become an engineer or a lawyer, and he ultimately received paternal approval for a scholarly career in history. Eventually training under Kurth and Fredericq at Liège laid the foundations for the wider experiences in Paris under Thévenin, Giry, Gauthier, and Fustel de Coulanges and later under Arndt at Leipzig, and for the challenge when faced by Harry Bresslau, Voigt, and Schmoller at Berlin, where he was once received by Ranke himself. Years of foreign study were rewarded by appointment at Liège in 1885 and to Ghent a year later. These prized positions were obtained only after months of anxious waiting and through influence exerted by family and friends. When politicians had the power to select candidates, politics, family standing and connections, religion, and language could easily outweigh training and aptitude for a university chair.

Could any career have brought greater rewards and recognition? Pirenne soon became known not only in his native Belgium but also in the wider circles of international scholarship. In 1912 during celebrations honoring his twenty-fifth year of teaching at Ghent, he made public confession of the favored life he enjoyed. Like a protagonist in a Greek tragedy, however, he had spoken too soon, for within a few months the Europe he believed he understood so well was forever destroyed. National and personal misfortunes loomed ever before him. The loss of a beloved son in the early years of war, life as a prisoner in Germany, disappointments and chagrin struck hard at him. Truly he was tested by fire, but the indomitable spirit always triumphed. Each crushing blow seemed to prepare him for the next misfortune. Yet how much he accomplished. He was the founder of the Belgian school of historical studies, the author of countless books and articles, and the leader in every phase of historical activities. By his magisterial *Histoire de Belgique* he forged the bonds of a nation, in his *Histoire économique et sociale* he produced an enduring classic, in his *Histoire de l'Europe* he gave his well-stocked mind free play to speak to a wide world of readers, and with *Les Villes du Moyen Age* he set the historical world on edge.

Mahomet et Charlemagne still challenges us.

Professor Lyon's rich and rewarding book is now surely the best introduction to Pirenne's work and place in historiography and the only sustained consideration of all aspects of his fruitful life. In his endeavor to assess the value of Pirenne's contributions for contemporary times Lyon helps us all. He is well aware that many of Pirenne's contentions are and will long be challenged and debated by scholars, but it is the boldness of concept, the daring execution, the succession of successes, the magnitude of Pirenne's achievements that remain and endure. One does not wonder that in his time he was acknowledged as the complete historian, the greatest of his time. It is the memory of this great scholar and noble man that this book will long keep alive.

GRAY C. BOYCE
Northwestern University

L. DE JONG. *Het Koninkrijk der Nederlanden in de tweede wereldoorlog* [The Kingdom of the Netherlands in the Second World War]. Volume 5, *Maart '41-Juli '42* [March '41-July '42]. Parts 1 and 2. (Rijksinstituut voor Oorlogsdocumentatie.) The Hague: Martinus Nijhoff. 1974. Pp. viii, 616; vii, 617-1171.

A. H. PAAPE, editor. *Studies over Nederland in oorlogstijd* [Studies of the Netherlands in War-time]. Volume 1. (Rijksinstituut voor Oorlogsdocumentatie.) The Hague: Martinus Nijhoff. 1972. Pp. viii, 399.

The fifth volume of de Jong's definitive history of the Netherlands during the Second World War covers the period from March 1941 to July 1942, from the end of the February strike in Amsterdam to the start of the deportations of the Jews. This periodization in itself emphasizes the importance of the persecution of the Jews in the evolution of the political climate during the occupation.

The present two-part volume strikes me as more clearly organized than the preceding parts. This may be due to the fact that certain broad themes dominated German actions in this period. The first of these is the attempt to win over the Dutch to National Socialism—which Seyss-Inquart considered one of his most important assignments—through the establishment of Nazi-type organizations and institutions in all walks of life, often under the control of Dutch Nazis. The second theme is the segregation of the Jewish population in preparation for their forthcoming deportation. In this connection it is worth noting that de Jong

claims that the top officials in the Reichskommissariat knew as early as the fall of 1941 that all Jews eventually were to be deported to the East. The third main theme of this volume deals with the rise of resistance. The author deals with this topic in separate chapters covering the churches, the world of art and literature, the underground press, and general resistance activities. He devotes an entire chapter each to the contacts with Dutch and British authorities in London and to military espionage and sabotage directed from London. In this context de Jong tells in detail the beginnings of the infamous Englandspiel, which enabled the Germans to arrest dozens of Allied agents dropped in Holland and to prevent all London-directed sabotage until late in 1943.

De Jong presents many points of information and interpretation that will be new to some readers. He claims that all high officials in the Reichskommissariat from the start supported the ultimate goal of integrating a National-Socialist Netherlands into the new Germanic Reich, disagreeing only on modalities, important as these may have seemed during the war. De Jong believes that Seyss-Inquart was quite conscious of his political failure by 1942 and prepared to delay until final victory another major offensive toward Nazification—this despite the fact that German and Dutch Nazi efforts to impose National-Socialist ideas and institutions continued until 1944. The author is very critical of the secretaries general for their willingness to collaborate with the Germans during this period in such matters as the issuance of identity cards, which gave the German police a tight control over the population; the proclamation warning Dutchmen against participation in resistance activity; and the collaboration of Dutch agencies in the early stages of the registration of Jewish persons and property. The tragedy of this collaboration “to avoid worse” reached its peak in the willingness of the Jewish Council to do all the paperwork for the Germans. This enabled the German police to organize the segregation and deportation of over one hundred thousand Jews with a staff of only twenty German officers. Here Hannah Arendt’s thesis about the Jewish Councils is substantiated in minute detail. All in all the present volume provides a well-integrated picture of this transitional period of the occupation.

The *Studies over Nederland in oorlogstijd* edited by A. H. Paape, a member of the staff of the Netherlands State Institute for War Documentation, contains seventeen reports that

were written over the past twenty-five years either as background studies for L. de Jong’s *Geschiedenis* or as *Gutachten* in connection with pending restitution affairs. Articles of general interest include a study of the goals of the German administration; sketches of two of the commanders of the *Sicherheitspolizei* in the Netherlands; a series of articles about disposal of Jewish property, including that of diamonds, a subject of special interest to Goering and Hitler; and finally an analysis of the claims made by Felix Kersten, Himmler’s masseur, after the war that he had singlehandedly saved the Dutch people from resettlement in Poland (this claim is found to be pure invention). These studies will be of value to scholars studying specific aspects of German occupation policies, especially those who are interested in comparative analysis. The value of the book to the American reader is enhanced by the fact that it contains English summaries.

WERNER WARMBRUNN
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GRETHE AUTHÉN BLOM *et al.* *Det nordiske syn på forbindelsen mellem Hansestæderne og Norden* [The Northern View of the Connection between the Hanseatic Cities and the North]. (*Det nordiske historikermøde i Århus, 7-9. August 1957.*) Edited by VAGN DYBDALH. 2d ed.; Aarhus: Universitetsforlaget. 1972. Pp. 195.

At the meeting of Scandinavian historians in Aarhus in August 1957, five persons gave papers summarizing the views that earlier and contemporary scholars held about the influence of the Hanseatic League upon Norway, Denmark, Sweden, Finland, and Iceland. Fifteen years later the demand for the volume that contained these essays led to a second edition, which added one page showing that the views which the five chosen scholars had presented had been little changed by the research and publication that had occurred since their utterance.

Attitudes toward the influence of the Hanseatic cities in Scandinavia show marked divergences. Grethe Authén Blom, dealing with Norway (pp. 1-54), finds that the pressure of German merchant groups kept down any attempts of Norwegian traders to rise to positions of power in overseas trade, while the long-line fishers north of Bergen were kept in virtual peonage. Nor was this exploitation compensated by the acquisition of cultural values by the Norwegians. Denmark, ably discussed by Aksel E. Christensen (pp. 55-96), was able

to keep a somewhat better balance in the struggle. Her political aims were time after time frustrated by the economic power of Lübeck especially, and by the measures taken by the German cities to protect their near monopoly of trade. There were, however, beneficial elements in the interchange, which aided the growth of Danish culture without subverting it.

Erik Lönnroth, in his treatment of Sweden (pp. 97-122), does a fine job of putting a great deal into a few pages. For Sweden, the Hanseatic League could be called into play against Denmark, against Mecklenburg, or against both. The economic strength of the Swedish merchants seems to have been fairly well maintained, and many of the Germans became natives in the cities where they traded. Culturally Sweden profited as much from German influences as she may have lost commercially. Two widely separated sections of Northern Europe gave rise to more favorable judgments upon the Hansa. Vilho Niitemaa, on Finland (pp. 123-64), and Björn Thorsteinsson, on Iceland (pp. 165-95), find that on balance the intercourse between the Hanseatic cities and their countries was of great benefit commercially, culturally, and socially.

All five writers emphasize that for the German traders there was only one standard by which intercourse with Norwegians, Danes, Swedes, Finns, and Icelanders was judged: "Did it pay?" Other standards may have existed, but no merchant city or its traders seem to have recognized their existence. The volume rightly emphasizes the positive values to Scandinavia of intercourse with the Hansa, but such dividends were incidental to commercial profits for the German merchants. It is from the Hanseatic centuries that we get the northern aphorism casually translated as: "What won't a German do for money, or a Swede for whiskey?"

The volume has for years been of the utmost value to students of the medieval and early moderate periods, and often a part of the material in graduate instruction and research. It is good that it has been reprinted in such usable form, and a translation into English seems called for to make it available to a wider circle of readers.

FRANCIS J. BOWMAN
Manson, Iowa

JENS ENGBERG. *Dansk finanshistorie i 1640'erne* [Danish Financial History in the 1640s]. (Skrifter udgivet af Jysk Selskab for Historie, 28.)

Aarhus: Universitetsforlaget. 1972. Pp. 356. 48.30 D. kr.

Jens Engberg's book makes a gesture at being useful to a broader circle of readers. The financial arrangements of the Danish-Norwegian state of the 1640s are briefly compared to the systems in effect at the same time in France, England, Sweden, Saxony, Brandenburg, and Holstein-Gottorp. Engberg also proclaims that financial history did not take place in a vacuum. The solid value of the book, however, will derive from the use given to it during the next half-century by the small number of scholars laboring in the same vineyard. For the specialist this book is going to be an indispensable reference work.

Engberg provides the reader, in the text as well as in numerous tables, an excellent and quite nuanced overview of the revenues that, during the 1640s, were at the disposal of the central authorities in the Danish-Norwegian state—roughly 1¾ million Riksdaler per year at the high point but falling to less than half that during the war with Sweden in mid-decade. In many cases, of course, the exact figures cannot be obtained, but Engberg's guesses are probably as informed as it is possible to be. More useful than the figures is the lovingly detailed description of the system and the people who, in the shifting combinations and offices of the antiquated Danish system, ordered, collected, supervised, spent, or audited the revenues. Most useful, however, is the painstaking commentary upon the source materials, their origins, their archival history and utilization, their limitations, and their best use. Engberg takes Hamlet-like delight in soliloquizing upon the complexities of his sometimes six or seven ways of arriving at a figure. The preferred method of calculating the value of income in kind, for example, emerges only after all alternatives are examined, and the results, soberly provided with caveats, are obviously less important than the delicious agony of the process.

The English summary, a dozen pages of somewhat random pluckings from the text, does little justice to the book. It is, of course, impossible to present in summary the thousands of big and little facts that make every page of this work a treasure, but it must be said that he who reads only the summary will be misled as well as shorted. As for defects within the Danish text and tables, aside from a few obvious typos, the reviewer without access to the sources cannot be critical. One could wish the author

had given as much attention to the expenditure side of the financial history as he does to the income side. One could wish he had condescended to sketch in a tiny bit more of the political picture for those readers who do not know about Christian IV's sons-in-law, his mistresses, the Torstensson invasion, and the setup with Schleswig-Holstein. It is clear, however, that those who really use this book do not need such background.

HEINZ ELLERSIECK

California Institute of Technology

STEN CARLSSON. *Ståndssamhälle och ståndspersoner, 1700–1865: Studier rörande det svenska ståndssamhällets upplösning* [Estate Society and Estate Persons, 1700–1865: Studies in the Dis-solution of the Swedish Estate Society]. Rev. ed.; Lund: CWK Gleerup, 1973. Pp. 414.

The first edition of *Ståndssamhälle* (1949) was a pioneering study in statistical social history. Professor Carlsson has now revised the book to take advantage of acceptable criticisms, to refute others, and to broaden somewhat his statistical base, while retaining the essence of content and methodology. He has "internationalized" the work by adding an excellent English summary (thirty-four pages) and English subtitles for fifty-odd tables and diagrams—although these are still not fully adequate for details and Swedish abbreviations.

Many of Carlsson's conclusions have already found their way into the general literature, but the research and the statistics are valuable to see in their original state. Even the famed demographic records of Sweden are not complete in every desired category, and changes in recording style often create difficulties. The author's explanations and adjustments are essential for a thorough grasp of problems and solutions.

The complex process of social change is revealed here, perhaps, with greater clarity than would be possible elsewhere because of the long-lasting formal rigidity of the estate system: nobility, clergy, burghers, and peasantry. The exclusivity of this social and political structure was slowly broken down by changes in Sweden's power status, the general atmosphere of ideas, economic developments, and the work of individual reformers. An essential factor in the erosion of the system was the steady increase in numbers and influence of "persons of standing" who belonged to none of the four recognized estates but who obtained education and position: people such as doc-

tors, professors, editors, officials, merchants, shipowners, and property holders. Since the late seventeenth century the "unprivileged" had acquired more and more tax-free land, and many of the nobility had become pauperized, while intermarriage and ennoblement had blurred the old lines of distinction. Again and again legal modifications simply sanctioned changes that had already occurred. The shift from the four-estate Riksdag to the two-chamber system in 1865 really had no immediate democratizing effect; it abrogated the obsolete estate principle and at the same time emphasized anew the importance of property.

This fundamental study has had a strong influence on Swedish historical scholarship, and it is good that it can now be known more widely.

FRANKLIN D. SCOTT
Honold Library,
Claremont

OLAVI JUNNILA. *Ruotsiin muuttanut Adolf Iwar Arwidsson ja Suomi (1823–1858)* [Adolf Iwar Arwidsson as a Finnish Emigrant in Sweden (1823–1858)]. (Historiallisia Tutkimuksia, 87.) Helsinki: Suomen Historiallinen Seura, 1972. Pp. 298.

Adolf Iwar Arwidsson was an early nineteenth-century Finnish nationalist intellectual who provided much of the ideological foundations for the consequent development of nationalist thought in his home country. Like so many other early nineteenth-century nationalists, he combined nationalism with a strong dose of liberalism in his political ideas. Not unexpectedly his public advocacy of such a combination of views brought Arwidsson difficulties with the bureaucracy of his time in Finland. His criticism of the strictness of censorship, the generally closed intellectual climate in Finland, and his championing of the rights of the subjugated Finnish-speaking majority and their language roused the ire of the dominant Swedish-speaking bureaucratic establishment. It caused the suppression of his newspaper, his dismissal from the University of Turku, and his subsequent move to Sweden. In that country Arwidsson had a successful career, eventually becoming the director of the Royal Library in Stockholm.

In spite of his move to Sweden, Arwidsson continued to write about Finnish affairs, often under various pseudonyms. It is this publicist activity in Sweden that forms the bulk of the contents of Olavi Junnila's book. The author

devotes much space—at times too much space—and ingenuity to details in establishing the various pseudonyms under which Arwidsson published his writings. He is able to show that Arwidsson's output and influence were far more extensive and varied, including publications in a number of European languages, than has been previously known. Arwidsson visioned ultimate independence for Finland, and his work as one of the earliest Finnish nationalist spokesmen has already attracted scholarly attention. These studies concentrated on his brief activity in Finland. Now Junnila has studied Arwidsson's years in Sweden, and in this book the author has shed some new light on a number of controversial issues in Finnish and Swedish nineteenth-century intellectual and political history.

PEKKA KALEVI HAMALAINEN
University of Wisconsin,
Madison

YVONNE HIRDMAN. *Sverges Kommunistiska Parti 1939–1945* [The Swedish Communist Party 1939–1945]. (Sverige under andra världskriget.) Stockholm: Allmänna Förlaget. 1974. Pp. 311.

The complicated, confusing, and taxing task of writing about the Communist party in any country frustrates historians and political scientists. A description of Communist party actions and strategies in Sweden during the war years proves no exception; it requires a road map to prescribe the turns, twists, and convolutions of party Politburo and leaders. Some members even "jumped off," rebelling at senseless dictation from the Communist International until its dissolution in 1943.

Swedish party leaders blamed England and France for initiating the "imperialist war," defended the Nonaggression Pact, and denounced Swedish "militarists" for defense preparations. The leaders castigated Finland for starting the Winter War in 1939, opposed Swedish aid, supported the treaty of March 1940 as a reasonable base for lasting peace between the Soviet Union and Finland, and charged England and France once again with "imperialist designs." They even, through the columns of *Ny Dag*, condoned German occupation of Denmark and Norway in 1940. After Hitler's invasion of Russia in 1941, the party completed an about-face to support the "war against facism," military preparations, and some violations of strict neutrality. Hirdman says that two types of strategies were designed for this period: attacking govern-

mental economic and postwar policies and joining in a form of a national front.

If these convolutions have not proven confusing, then Hirdman's detailed study can be reasonably followed. Her narrative description of reasons for the party's policies, its responses to events, and leadership roles in an excellent digest of these years. More life might have been infused into the chronicle by some indications of inner-party conflicts, rabid anti-Communist attacks on them, or glimpses of the leaders' troubled lives. The horrid arson murder of five Communists in burning offices of *Norrskensflamman* is passed over quickly, as are efforts of government and private groups to obstruct the party. Leaders seem only automata, only partially true to life, and rebels are not mentioned. Another failure, despite summaries and an excellent English abstract, is the lack of perspective that results from weak analysis. The writing is stiff, the index ridiculous, and the notes and bibliography are extensive. On the whole, this addition to the series on Sweden during World War II furnishes new materials in depth.

RAYMOND E. LINDGREN
California State University,
Long Beach

EVA-CHRISTINA MÄKELÄINEN. *Säätyläisten seuraelämä ja tapakulttuuri 1700-luvun jälkipuoliskolla Turussa, Viaporissa ja Savon kartanalueella* [The Social Life and Customs of the Gentry in Late XVIIIth-Century Turku, Sveaborg and the Savo Estates Area]. (Historiallisia Tutkimuksia, 86.) Helsinki: Suomen Historiallinen Seura. 1972. Pp. 270.

Life during the late 1700s among Finland's privileged classes—nobility, officialdom, wealthy merchants, some academicians and intellectuals—was varied, patently pleasurable, and distinctively French in aspiration. Eva-Christina Mäkeläinen, who displays a keen eye for colorful and apt quotations taken from contemporary correspondence, diaries, and memoirs, has succeeded in presenting a delightful and well-rounded portrait of how Finland's elite lived: their attire and appearance; household furnishings and libraries ("How many read novels [in French, of course!]" and allow the porridge to burn," was one complaint); weddings, baptisms, and funerals; celebration of Christmas, Midsummer Eve, and other holidays; balls, soirees, and assemblies; card playing and the use of tobacco. Of unusual importance are Mäkeläinen's materials on the rise of musical and theatrical pursuits in Finland.

The characteristic patterns of upper-class life were derived chiefly from Sweden, where French influences during the Gustavian Age were dominant. They were mediated to the Eastern Province by Finns serving in the royal establishment, Swedes sent on various missions to Finland, the visits of the Swedish monarchs, and through other contacts. Often cultural activity was stimulated by influential personalities: Henrik G. Porthan, Frans M. Franzén, and A. N. Clewberg-Edelcrantz in Turku, Augustin Ehrensward in Viapori, and Yrjö Mauno Sprengtoporten and Samuel Möller in Savo.

But Turku, Viapori (Sveaborg), and the manorial estates of the Savo region, which are the focuses of this study, were not in the class of even a miniature Stockholm or Paris. Finland was a remote, peripheral, and relatively poor and backward province; contacts with the mother country and the continent were intermittently broken or delayed; there was no local court to serve as a nucleus for an inimitable and lasting way of life. Foreign influences came irregularly, belatedly, and in diluted form. While they may have given a momentary esoteric dimension to Finnish life, their impress in the long run was ephemeral.

A detailed bibliography and an English-language summary add to the value of this first-rate contribution.

JOHN I. KOLEHMAINEN
Heidelberg College

ROBERT N. CROSSLEY. *Luther and the Peasants' War: Luther's Actions and Reactions*. (Exposition-University Book.) New York: Exposition Press, 1974. Pp. xii, 164. \$8.00.

This study examines Luther's actions during the critical years after 1521 when the strength of the young Reformation and the leadership of the Reformer himself were tested. Three uprisings—the Wittenberg Disturbances of 1522, the Knights' Revolt of 1522–23, and the Peasants' War of 1524–25—are studied to show Luther's theological, political, economic, and social views in the face of rebellion. In general, the results are favorable to Luther, emphasizing the consistency of his views and behavior and answering the critics of the Reformer's actions and utterances. In areas other than the theological, the author is less successful in his attempts to show why Luther acted as he did. Whether Luther's indictment of the rebellious peasants was motivated in part because he was of the middle class and feared destruction of his own lands and property is doubtful.

The author rightly emphasizes the theological basis for Luther's views on rebellion, although chapters are devoted to Luther's conservative economic and political views. Returning to Wittenberg from the security of the Wartburg in March 1522, Luther in eight sermons quelled the disturbances that Karlstadt and the Zwickau Prophets had incited. Although he was in agreement with many of the proposed religious reforms, he was strongly opposed to reform by force. Luther also disassociated his religious movement from the political demands of Sickingen and his associates in the Knights' Revolt. The Reformer's role in this uprising was so minor that the chapter on the revolt adds little to the thesis of the book.

In the more extensive treatment of Luther's actions during the Peasants' War, the unorganized revolt, Luther's mild reply to the Twelve Articles, his exhortation to peace, and his intemperate and blunt condemnation of the excesses of the fanatic peasants are analyzed. Luther was consistent throughout in his insistence that the citizen, no matter how abused, must obey the government, divinely instituted to maintain order and punish the guilty.

This work, somewhat redundant, is probably best described as semipopular. This is also evident in the bibliography of titles cited, useful titles not cited, and other works useful to the reader. The German Wittenberg Ordinance of 1522, the Twelve Articles in English, and a brief index of names complete the work.

KARL H. DANNENFELDT
Arizona State University

HEINZ MOSCHE GRAUPE, editor and translator. *Die Statuten der drei Gemeinden Altona, Hamburg und Wandsbek: Quellen zur jüdischen Gemeindeorganisation im 17. und 18. Jahrhundert*. Volume 1, *Einleitung und Übersetzungen*; volume 2, *Texte*. (Hamburger Beiträge zur Geschichte der deutschen Juden, number 3, volumes 1 and 2.) Hamburg: Hans Christians Verlag, 1973. Pp. 344; 293. DM 76 the set.

I have mixed feelings when approaching a book of this type. Thirty years ago a German government expended a great deal of effort in the physical destruction of Jewish men, women, and children. Now a German government subsidizes historic research into the Jewish past in Germany. We have preserved the archives but not the human beings.

The work in question consists of two volumes: one contains the transcription of the original Hebrew and Yiddish texts of the statutes as they have been preserved in the manu-

scripts; the other consists of the German translation of these materials together with a well-written, sixty-three-page introduction by Professor Graupe.

Until the early 1800s, the Jewish communities of the Diaspora served as the religious, social, and administrative center of Jewry. Among these functions were included organization of religious services, civil jurisdiction, education, welfare, care of the sick, guardianship for orphans, ritual matters, cemeteries and burial responsibilities, commercial and marital relations, probate, fiscal allocations and collections, and relations with the non-Jewish environment and with the governmental authorities. The great expenditure, constantly necessary for internal and external purposes, forced the congregational officers to limit full membership to people of means, and exclude the poorer Jews from full rights and participation in communal affairs.

The seventeenth- and eighteenth-century statutes of the three congregations—Altona, Hamburg, and Wandsbeck—are of special significance since they belong to the most comprehensive and detailed of such collections. Since these 1,015 folios were entrusted to the archives of the city of Hamburg in 1938–39 rather than to the Berlin archives, they escaped destruction during the war. During the period in question each of the three communities rewrote its rules and regulations at least once; since the same chief rabbi and rabbinical court served the three congregations, many similarities in customary procedures and environmental influences are reflected (for example, the many, apparently hopeless attempts to control ostentatious clothing and wedding celebrations, or stemming participation in “modern” social activities by the young).

Primary materials such as those presented in these volumes should be of value to historians, sociologists, economists, and legal scholars.

FRANK ROSENTHAL
University of Judaism

GERALD LYMAN SOLIDAY, *A Community in Conflict: Frankfurt Society in the Seventeenth and Early Eighteenth Centuries*. Hanover, N.H.: University Press of New England, for Brandeis University Press. 1974. Pp. xvii, 252. \$12.00.

Franklin Ford's *Strasbourg in Transition* serves as a standard for studies of urban societies in the early modern period. It does Gerald Soliday no injustice to characterize his monograph as derivative from Ford, who was his

thesis adviser. Soliday has a narrower focus, both because of the general work already done by earlier historians of Frankfurt and because of the archival losses World War II inflicted on the city. Specifically, he uses the constitutional conflict of 1705–32 to study Frankfurt's social structure and to weigh the impact of social conflict on that structure.

Two kinds of conflict marked Frankfurt's history during this period: a struggle for power within the citizenry between the urban aristocracy and the burghers and a struggle between the citizens and the denizens, permanent inhabitants of the city who did not have citizenship rights. The latter conflict finally moderated the former, reinforcing Frankfurt's traditional corporate structure by consolidating citizen solidarity against outsiders. This result sustains Soliday's conviction that Weber's categories of class, status group, and party remain as the most viable interpretive scheme for understanding Frankfurt in the seventeenth and eighteenth centuries. Accepting Otto Brunner's emphasis on the corporatist framework of German urban society, Soliday concludes that status was a more fundamental social factor than class in this period.

Soliday also makes a number of particular observations about Frankfurt: that despite the length and intensity of the constitutional conflict, the burghers never demanded the elimination of aristocratic power or radical changes in the social structure; that the essential achievement of the conflict was to establish effective limitations on the city council and on the aristocracy that controlled it; that Lutheran pre-eminence in the power structure remained a vital concern; and that in all conflicts the support of the imperial government was decisive in determining the outcomes. Beyond these, Soliday also provides a graphic description of Frankfurt society.

Though derivative—even Soliday's use of Weber and Brunner shows his mentor's influence—this monograph provides an admirable introduction to the complexities of German urban history prior to the French Revolution. The intelligent blend of basic archival research and theoretical constructs itself should serve as a standard for similar works.

WILLIAM J. MCGILL
Washington and Jefferson College

CARL HAASE, *Ernst Brandes, 1758–1810*. Volume 2. (Veröffentlichungen der Historischen Kommission für Niedersachsen und Bremen, 32. Niedersächsische Biographien, 4.) Hildesheim:

August Lax Verlagsbuchhandlung. 1974. Pp. x, 580.

This lengthy second volume, which treats the final years of Brandes's life, from 1791 to 1810, completes the intellectual biography of a minor though representative figure of early enlightened conservatism in Germany. Really a supplement to the already tedious first volume, it is of no value to the general historian and of marginal interest to specialists interested in Hanoverian history, bureaucracies, and the state of public opinion during the revolutionary and Napoleonic period. This is a pity, as the author based this labor of love on much archival work and voracious reading of Brandes's writings. What ensued, however, is a rambling and uninteresting book that adds little to our knowledge of the period. It confirms the suspicion I had upon completing the reading of the first volume, that it would have been kinder to leave Brandes in the oblivion his personality and thought merit.

FRANZ NAUEN
University of Haifa

HORST DIPPEL. *Deutschland und die amerikanische Revolution: Sozialgeschichtliche Untersuchung zum politischen Bewusstsein im ausgehenden 18. Jahrhundert.* (Inaugural-Dissertation zur Erlangung des Doktorgrades der Philosophischen Fakultät der Universität zu Köln.) Cologne: [the author.] 1972. Pp. xxx, 208.

Dippel attempts to determine what effect the American Revolution had upon Germany, and, in particular, what social ramifications occurred in Germany because of that Revolution. This exhaustive work shows that the American Revolution did not promote radical changes in Germany.

Germans knew practically nothing about the colonies before 1770; they learned about a few things afterward, and, consequently, their understanding of the Revolution was slight indeed. On this ominous note Dippel proceeds methodically to describe the German reaction to the crisis as it developed between 1770 and 1774, the outbreak of fighting, and the growing difficulties of the English government in crushing the revolt. Washington was characterized by many as another Quintus Fabius, Franklin as the intellectual wise man, and the colonists as lovers of "freedom" and "equality." Afterward, German commentators remarked that the Articles of Confederation did not provide a strong central government but were nearly

silent concerning the Constitution that followed. Of course, as Dippel mentions, by 1788 and 1789 they were more and more concerned about the French Revolution and neglected the United States.

The German commentators fell into prorevolutionary and antirevolutionary groups. Those in favor of it tended to lard their discussions with tiresome platitudes about the generous nature of Americans and their benevolent leaders. Antirevolutionary Germans complained of "anarchy" and characterized the colonists as wrong-headed rebels. Republicanism and other principles of the Revolution were interpreted in the light of the German Natural Law or by analogy with the Dutch or the Swiss republican movements. In all cases, neither the friends nor the foes of the American Revolution in Germany made profound comments on that Revolution. What was the end result? Perhaps a majority of literate Germans, mostly bourgeois, like the American government after 1783, thought North America was an Elysian field, thought it would be nice to be "free" and "equal," but did nothing to change their own society. Only with the coming of the French Revolution would some of these enthusiasts, such as the Mainz revolutionaries, find an opportunity for change.

The conclusions of Dippel support those of R. R. Palmer and others. The result of this enormous labor is thus extremely modest.

HUBERT C. JOHNSON
University of Saskatchewan

DIETRICH THRÄNHARDT. *Wahlen und politische Strukturen in Bayern 1848-1953: Historisch-soziologische Untersuchungen zum Entstehen und zur Neuerrichtung eines Parteiensystems.* (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 41.) Düsseldorf: Droste Verlag. 1973. Pp. 360. DM 78.

If sheer technical competence made a superior book, this would be a superb study of political structure and practice in Bavaria since that state began the long march of modernization. Dr. Thränhardt, a student of the late Waldemar Besson at the new University of Constance, manages his statistics, charts, and electoral profiles with obvious skill; and he displays a thorough acquaintance with the analytic techniques generated by American political science and political sociology. He largely succeeds, moreover, in relating these matters to Bavaria's unique circumstances and problems as they evolved over a century. He has performed no

small feat in avoiding both the elaboration of empty generalities and the recitation of idiosyncratic details. In the presentation of his evidence the author is consequently able to give a consistent and usually convincing account that does justice to the complexity of what is still too often written off as "Catholic Bavaria." Yet the entire enterprise lacks a perspective that might have freed it from the category of a competent dissertation.

Thränhardt divides his subject into three chronological blocks: from the revolution of 1848 to the outbreak of the First World War; from the onset of the Weimar Republic to the collapse of the Third Reich; and the postwar reconstruction until 1953. Of these the first is somewhat weakened by insufficient attention to the interaction of bureaucratic agencies and political factions; the second is unobjectionable, though not especially enlightening, as to the impact of nazism; and the third, the heart of the book, remains curiously incomplete. Throughout there is a relentlessly mechanical quality to the explanations of political behavior that convey little sense of the atmosphere under the Wittelsbach monarchs, republican officials or Nazi *Gauleiters*, and the American forces of occupation. In the closing sections the author dwells on the emergence of a new political system in the midst of the cold war, but he is content to leave matters at that. The book conspicuously offers no summary conclusion; it just ends. As a result the reader is left with no inkling of the most significant changes in Bavaria's economic and social composition since 1945: the rapid development of technological enterprise and the creation of an extraordinary prosperity in what had formerly been, for the most part, an agricultural backwater of Germany's industrial establishment.

These virtues and deficiencies leave the impression of a work well conceived, diligently executed, and yet not entirely conclusive.

ALLAN MITCHELL
University of California,
San Diego

LARRY H. ADDINGTON. *The Blitzkrieg Era and the German General Staff, 1865-1941*. New Brunswick: Rutgers University Press. 1971. Pp. xvii, 285. \$10.00.

There are two ways in which this book can be read. One is as a lucid and concise history of the initial phase of the Second World War, from the Polish campaign to the battle of Moscow in the winter of 1941. The other is

as a book with a thesis. The thesis is that the Blitzkrieg theory of Hitler's army was not a new one but rather "the final step in the evolution of the old nineteenth-century Prussian doctrine of *Kesselschlacht*"—the battle of quick encirclement and annihilation. The principal modifications were those designed to take advantage of new weapons, such as the airplane and the tank, and to allow for greater mobility, for freedom from the tyranny of railroad timetables, by the use of motorized divisions and motorized logistics. The German planners had the lessons of World War I very much in mind. They knew that any future campaign would have to be won quickly, that another war of attrition was one that Germany was likely to lose again.

The thesis, too, is that for all the soundness of the Blitzkrieg strategy, and for all the competence of the men carrying it out, the Germans had not suddenly turned into military supermen, able to defeat in lightning campaigns all those who dared oppose them. The new version of the *Kesselschlacht* idea "proved sound in those areas for which the doctrine was originally developed—Central and Western Europe—and against states with common land frontiers with Germany and with relatively small land areas in which enemy forces could retreat or maneuver. But the 1939 doctrine was no better than that of 1914 in respect to anticipating requirements for waging war successfully against an insular country such as Great Britain or a continental power such as Soviet Russia. The Blitzkrieg army and its supporting Luftwaffe lacked the range, versatility, resources, and naval support to defeat such enemies."

But no matter how the book is read, whether for its survey of the first two years of the war or for its thesis, it is fair and factual and objective. The only possible reservation, and it is a pedantic one, is that since the book is based to a large extent on the Halder papers, there is a certain tendency to be sympathetic to the chief of the general staff, but then it would probably take more than another *Kesselschlacht* to imagine a book that would spend that sympathy on his commander in chief.

JOACHIM REMAK
University of California,
Santa Barbara

RUDOLF LILL. *Die Wende im Kulturkampf: Leo XIII., Bismarck und die Zentrumsparlie, 1878-1880*. Tübingen: Max Niemeyer Verlag. 1973. Pp. xx, 228-82, 658-729. DM 18.

This important study is a special printing of a two-part article appearing in volumes 50 and 52 (1972) of *Quellen und Forschungen aus italienischen Archiven und Bibliotheken*, providing a summary and interpretation of the first volume of published documents from the Vatican Archives dealing with the Kulturkampf. It is this first thorough use of the Vatican sources that gives the piece its special significance. The author, who has published a number of studies on the Kulturkampf, also edited the documents (*Vatikanische Akten zur Geschichte des deutschen Kulturkampfes, Leo XIII., vol. 1, 1878-1880* [1970]), and his basic view is that through a combination of cautious negotiation, flattery, trickery, and brutal pressure Bismarck in this first stage forced Pope Leo XIII to abandon his illusions of a high-level treaty on the basis of equality between two sovereign powers and to accept Bismarck's own piecemeal, pragmatic, discretionary approach, which left the essential legal supremacy with the state. Both pope and chancellor wanted to bring the church-state struggle to a close and to create an organic association between the two to maintain conservative values against the socialist threat, but the chancellor was determined to do this without sacrificing the principle of state political supremacy achieved in the seventies. The study is especially valuable for its clear delineation of the relationships among curia, bishops, and Center party leaders. Although caught between an authoritarian hierarchy in Rome, which basically distrusted its democratic parliamentary orientation, and a ruthlessly autocratic dictator in Berlin, the Center party nevertheless, from the sheer overall logic of the changing political situation, gradually emerged in a much stronger position in relation to both Berlin and Rome. Because of the high-level, political nature of the process of amelioration of the Kulturkampf, the author believes that the authority of the Roman curia over bishops and local congregations actually increased, ironically, since the ostensible enemy of the liberal state had been "ultramontanism."

J. ALDEN NICHOLS
University of Illinois,
Urbana-Champaign

HUBERT JESCHKE. *U-Boottaktik: Zur deutschen U-Boottaktik 1900-1945*. (Einzelschriften zur militärischen Geschichte des Zweiten Weltkrieges, number 9.) Freiburg: Verlag Rombach. 1972. Pp. 120.

In the U-boat the Germans had a weapon that might well have defeated England in both world wars. Jeschke, a career naval officer, documents and explains the incredible failure to exploit what really could have been a *Wunderwaffe*. Indeed, the reader closes the book shaking his head in wonderment that the Germans managed to inflict as much damage as they did. The same two errors were fatal in both conflicts.

The first was lack of coordinated command. In World War I this took the form of disunity within the navy itself. More than a dozen naval agencies enjoyed the right of reporting directly to the Kaiser. Under these circumstances it proved impossible in 1917-18 to ram through the new strategy, advocated by many officers, of combining U-boats for group attack. To the astonishment of the English, the convoy system had been in effect for an entire year before the Germans made their one and only such attack. In World War II, on the contrary, "wolf packs" were employed from the beginning, but this time disunity of command lay at a higher level. Though the navy was now in agreement on its strategy, it failed in the competition with the other armed services to persuade Hitler to give Dönitz the resources he needed or to let him fully concentrate the ones he had.

The other error was simply the failure to build an adequate number of submarines. Though unrestricted U-boat warfare was proclaimed on February 1, 1917, not until December was a substantial order for submarine construction even placed, much less filled. Despite this experience, in the second conflict the Germans again waited until 1943, when the war was already lost, before giving belated attention to mass U-boat production. In October 1940 Dönitz had only eight submarines in the battle zone around England! Thus General Hoffmann's acid verdict applies as well to the second war as to the first: this was "unrestricted U-boat warfare—without U-boats."

ROBERT E. NEIL
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WILLIBALD GUTSCHE. *Aufstieg und Fall eines kaiserlichen Reichskanzlers. Theobald von Bethmann Hollweg, 1856-1921: Ein politisches Lebensbild*. Berlin: Akademie-Verlag. 1973. Pp. 267. DM 11.80.

One focus in the "Fischer controversy" is the role of Chancellor Bethmann Hollweg. Zechlin and Vietsch have portrayed him as humane, pacific, and democratic. Jarausch sees him as the enigmatic victim of society and of his

character. Gutsche's interpretation approaches Fischer's, though with a special twist; the "first detailed Marxist-Leninist biography" begins and ends with a quotation from Karl Liebknecht. An impressive array of evidence stresses Bethmann's concern with economic forces. If this approach sometimes lacks subtlety, it is illuminating to picture him against an industrial bourgeois, rather than Junker, background.

"Joyless, pessimistic and doubting," Bethmann, opposing both reactionaries and democrats, worked as a moderate for the development, not the alteration, of the existing system. Seeking the support of the upper bourgeoisie, he endeavored to reconcile monarchy with the workers and mute class conflict. Domestically this meant striving for Prussian suffrage reform and suppressing the Social Democrats. Here he was a conspicuous failure.

Nor, in Gutsche's view, was he any more successful in foreign affairs. He desired British neutrality but was unwilling to pay the price of sacrificing the fleet and German plans for indirect hegemony in Europe, which was necessary for its achievement. He backed an economic *Mitteleuropa* and *Mittelafrika*. He helped precipitate World War I by readiness to take Balkan risks. Knowing Hindenburg and Ludendorff were outright annexationists, he aided them to power. Token counterproposals were his sole response to peace feelers in 1916-17. He ultimately favored U-boat warfare, realizing its consequences. Throughout, the interpenetration of foreign and domestic policy is stressed.

Bethmann's entire life and role in imperial Germany does need re-examination, but this intention is not always implemented. Concentration on the present controversy leaves gaps in the biography. It is significant, however, that Bethmann early combated national minorities and Social Democrats and opposed using pressure on capitalists for social reform while supporting colonialism and navalism.

This clearly written study, accurate on details, rests upon extensive use of printed and archival sources. It will be cited often in the continuing debate over the nature, aims, and actions of Wilhelmine society.

FREDERIC B. M. HOLLYDAY
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JOHN G. WILLIAMSON. *Karl Helfferich, 1872-1924: Economist, Financier, Politician*. Princeton: Princeton University Press, 1971. Pp. xviii, 439. \$13.50.

Karl Helfferich's life was a finely balanced dialectic of the constructive and the destructive. Advancing the Berlin-Baghdad railway as bureaucrat and banker, he served Germany's imperialistic thrust with efficiency and energy. It never occurred to him that in unison with many other members of the German elite he was playing at power politics more seriously and thus more dangerously than his emperor. After the war began, he served his country's doubtful cause successively as state secretary (minister) of the treasury, state secretary of the interior, and vice-chancellor. If he incomprehensibly compounded Germany's greatest errors, he was otherwise so sensible, and so arrogant about it, that he made enemies ranging from the military to the social democracy and was shunted away from power in the last year of the war. During the early years of the Weimar Republic, he became a leader of the Nationalists and drove them against their anachronistically monarchist principles and toward moderation and republican responsibility. The last of his labors before his accidental death in 1924 was the domestic monetary strategy that led Germany out of the great inflation. He also engaged in violent polemics that exacerbated the country's divisions and encouraged the assassinations of Erzberger and Rathenau. His life and works led to parliamentary democracy and nazism.

Williamson has conscientiously collected enough hard data to stop future historians from writing off Helfferich as a dead loss to German history. The biographer, however, does not trust his own evidence, too often finding his man guilty when the facts argue for a Scotch verdict. He also makes the mistake of taking Helfferich's words more seriously than Helfferich did. Thus Williamson condemns Helfferich's criticism of the Dawes Plan as negative. Yet this was precisely the way Helfferich maintained contact with his balky party and posthumously kept it from blocking acceptance of the plan, while his currency proposal was important in making the plan possible. Williamson has rescued Helfferich from simple obloquy only to let him slip into qualified obloquy.

Williamson's concentration on the question of guilt or innocence narrows the interpretation of Helfferich's life and times almost to the vanishing point. As far as the times are concerned, Williamson has contented himself with hauling out the battered stage sets of Wilhelmine Germany as an officers' paradise and industrialists' field for cartel building, and the

Weimar Republic as the scene of a Manichean civil war.

With all his contradictions, Helfferich could be a fascinating subject for a biography. Although Helfferich left few private papers, he revealed much of himself in his many scholarly and polemical writings and in his eloquently rationalized acts. Williamson is tone-deaf to the personal note. His Helfferich is a mechanical man and a bore.

At least Williamson, properly taking Helfferich as a model for thoroughness, has contributed a well-organized body of facts to the historiography of modern Germany. His findings, scantily clothed with his interpretation as they are, invite other studies.

DAVID FELIX

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WINFRIED BAUMGART, editor. *Von Brest-Litovsk zur deutschen Novemberrevolution: Aus den Tagebüchern, Briefen und Aufzeichnungen von Alfons Paquet, Wilhelm Groener und Albert Hopman, März bis November 1918*. With a foreword by HANS HERZFELD. (Deutsche Geschichtsquellen des 19. und 20. Jahrhunderts, number 47.) Göttingen: Vandenhoeck & Ruprecht, 1971. Pp. 750. DM 118.

With this edition of the diaries and letters of journalist Alfons Paquet, General Wilhelm Groener, and Admiral Albert Hopman, Winfried Baumgart has enhanced substantially his reputation as an expert on German eastern policy between Brest-Litovsk and November 1918. The study ably provides three contrasting aspects of the interaction between the Bolshevik Revolution and German policies in Russia.

Paquet, in his position as reporter to the *Frankfurter Zeitung* and press attaché to the German legation, maintained close contact with pivotal Bolshevik figures in Petrograd and Moscow. His diary entries clearly mirror the impact of the revolution upon this open-minded intellectual. From impartial and neutral observer, Paquet gradually became a Social Democrat and admirer of a revolution he regarded as the greatest slave uprising in history. Despite the revulsion evoked in him by the barbarism of bolshevism, he believed, like Lincoln Steffens, that he had seen the future and that it worked.

The notes of Hopman and Groener present two less sanguine viewpoints; both totally opposed bolshevism and advocated its destruction before it could take permanent root. Yet Hopman, who had been sent to the Crimea to re-

store shipping in the Black Sea, also viewed German foreign policy since Bismarck as an abysmal failure and was especially dubious about Ludendorff's overweening expansion policy. Groener, who had been detached to Kiev to support the Rada and to organize grain shipments, was similarly critical of Ludendorff. Recognizing the chimerical nature of an independent Ukraine, Groener recommended a strengthening of German forces in the east in order to create reality from illusion, unaware of Ludendorff's actual intent to reduce army strength in the eastern theater. Ultimately Ludendorff lost on both counts, as Germany gained neither sufficient manpower for the western offensives nor the anticipated grain from the east. Additionally, Groener's diary informs the reader of his contempt for civilians, politicians, diplomats, and Germany's allies, a view so representative of the caste that contributed to Germany's bankruptcy.

In the preparation of his meticulously edited volume, Baumgart has produced an indispensable source study of this critical period of history.

PETER BECKER

University of South Carolina

JANUSZ SOB CZAK. *Propaganda zagraniczna Niemiec weimarskich wobec Polski* [Foreign Propaganda of Weimar Germany against Poland]. (Studium Niemcoznawcze Instytutu Zachodniego, number 24.) Poznań: Instytut Zachodni, 1973. Pp. 355. Zł. 65.

The important part played by the "Polish scare-crow" in the internal politics of the Weimar Republic is generally well known. But this is the first extensive study focusing on the anti-Polish character of German revisionist propaganda carried abroad by Germany during 1919-33. It is based mainly on German archival material, and it clearly establishes the institutional and personal ways and means through which anti-Polish propaganda abroad was directed and financed by Germany. The book is organized into six chapters covering such topics as premises of Weimar policies toward Poland; instruments, techniques and means of German propaganda, its content, themes, slogans, specific propaganda campaigns, and countries where it was disseminated. The author concludes that Weimar propaganda for revisions of German borders with Poland was not overly successful, and some of its early successes were cancelled out by the aggressive policies of Nazi Germany.

The book makes heavy reading, and it is repetitious in some places. It describes scrupulously and convincingly the intricate machinery and the slanderous ways of the German anti-Polish propaganda and the importance attached to it by those in the position of power in the Weimar Republic. But it is short on analysis and gives us little insight into how the author arrived at his final conclusion. The book is, however, a valuable and dependable source of information on the subject.

CHESTER M. NOWAK
Bridgewater State College

JÜRGEN C. HESS. *Theodor Heuss vor 1933: Ein Beitrag zur Geschichte des demokratischen Denkens in Deutschland*. (Kieler Historische Studien, number 20.) Stuttgart: Ernst Klett Verlag, 1973. Pp. 230. DM 36.

Critical of the flaws in imperial Germany and eager for reform but committed to a gradualism similar to the political development in England, the German liberals were caught unprepared by the German defeat in 1918 and the revolution that followed. Their transformation toward the system, from critics to defenders, from admonishers to protectors, and, saddest of all, from political outs to ins, was at best difficult and in many cases impossible.

The political thinking of Dr. Theodor Heuss was no exception. His writings in the early part of the 1920s indicate how comfortable the liberals had become in the context of imperial Germany and how difficult they found the new republic. But Heuss made the transformation; although he detested revolution, was shocked and humiliated by Versailles, and found the task of working with the socialists more difficult than he had imagined, he soon became a sincere and dogged supporter of the Weimar Republic.

In a clearly written, well-organized dissertation, Jürgen C. Hess has outlined the political thinking of the first president of the Federal Republic before the advent of Hitler's Third Reich. Ignoring many facets of Heuss's career, such as his role as a Reichstag deputy, Hess has organized a coherent and stimulating presentation of Heuss's political thinking from his speeches, lectures, letters, and writings. The picture of Heuss which emerges is that of a coolly rational, moderate intellectual on the periphery of political decision making but an intellectual deeply committed to making the republic a viable institution. Hess concedes that Heuss had not the systematic and incisive political think-

ing of his friend and mentor Friedrich Naumann, but he was more the intellectual promoter of liberalism. Like most German liberals, Heuss envisioned a parliamentary state for Germany that had the respect and authority of imperial Germany but also one that could successfully integrate all elements of German society, including the working class, into its polity. Heuss saw the need for responsible leadership and party politics, but he was too much the liberal not to be offended by the party excesses in Weimar and the search for the leader by the right-wing. In foreign affairs he advocated a strong Germany with equality in arms backed by a conscripted army, a greater Germany with the inclusion of Austria, and a balanced Europe. Under the slogan, "democratic nationalism," Heuss sought to rally the loyalties of all Germans to the new republic, but by necessity the republic had to be a strong state with a decisive foreign policy.

Although Hess obviously admires Heuss, the author is not blind to his faults. In the chapter dealing with Heuss's rejection of National Socialism, the author points out how Heuss typically underestimated and, in many ways, completely misjudged the nazism in 1932. In a disappointingly written chapter, Heuss's concepts of a "human economy" and economic planning seem hesitant and incomplete. In general, however, Hess's sympathetic monograph contributes much to our understanding of the development of this Swabian educator of German democracy.

EDWARD L. HOMZE
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Lincoln

STANLEY SUVAL. *The Anschluss Question in the Weimar Era: A Study of Nationalism in Germany and Austria, 1918-1932*. Baltimore: Johns Hopkins University Press, 1974. Pp. xxi, 240. \$12.50.

This book, the "first comprehensive look at the Anschluss question from both German and Austrian perspectives," approaches its topic from four aspects: as an important component of German revisionist foreign policy; as involving domestic as well as foreign policy considerations; as playing a much greater role in Austria than in Germany, because in the latter it was only one of several revisionist goals; and, finally, as a model for the study of German nationalism during the Weimar era. As to the last point, the author stresses the need to treat

nationalism not as a monolithic phenomenon, but to recognize the pluralistic characteristics of nationalism which "scholars have consistently underestimated." But is the latter assertion true? Which student of the German unification movement of the nineteenth century, for example, could overlook the pluralistic character of nationalism? As in the four main approaches to his study, Mr. Suval all too often belabors the obvious, which together with all too many repetitions, makes the reading of this book somewhat arduous.

More serious, the study lacks, in the humble opinion of this reviewer who has spent all the years of the Weimar era in Germany and Austria, a sure sense as to the relative importance of persons and events. Everything is treated with the same intensity. For instance, it is hard, from reading Suval's book, to form a judgment as to the relative importance or lack of it, of the Pacifists, the Pan Europe, and other movements. Persons like Heinrich Mataja and Otto Bauer are treated on the same level, so to speak. The importance of the mass demonstrations on the occasion of the Schubert Festival in 1928 is vastly overrated. So are the propaganda abilities of the adherents of *Anschluss*. ("The Anschluss propagandists were as skillful as any of the minions in Goebbel's ministry" [p. 179].) On the other hand, the constant mass unemployment in Austria during the whole period is barely mentioned even though it was probably the most powerful factor, and certainly not merely a psychological one, which convinced so many Austrians that their state was not viable.

A most impressive amount of research has gone into Suval's study. The results are most satisfactory in the chapter on the German-Austrian Customs Union project and the one dealing with the German (and Austrian) historians' attempt to find a place for Austria in German "national symbolism." There is the normal amount of inaccuracies. For example, Ernst Karl Winter was not made major of Vienna by Schuschnigg, but third vice-major by Dollfuss. Otto Bauer resigned as foreign minister in 1919, not 1920, and not he but Renner greeted the *Anschluss* in 1938. Julius Meinel was not a Viennese lawyer, but a prominent businessman. Also, careful proofreading by somebody competent in German might have avoided the frequent misspellings and other irritating distortions in German titles cited in the footnotes.

Much as there is to this book, it suffers from the fact that during most of the period "the Anschluss Question was not acute." This was

a fact rather than "the solution found by ingenious diplomats to consolidate the ambiguities within the German position" (p. 145). Reading Suval's study left serious doubts in my mind whether the *Anschluss* question during the Weimar era can and does provide a meaningful model for the study of German nationalism.

ERIC C. KOLLMAN
Cornell College

JOACHIM C. FEST. *Hitler: Eine Biographie*. Berlin: Propyläen Verlag Ullstein. 1973. Pp. 1190. DM 38.

JOACHIM C. FEST. *Hitler*. Translated from the German by RICHARD and CLARA WINSTON. New York: Harcourt Brace Jovanovich. 1974. Pp. xiii, 844. \$15.00.

There has been an avalanche of books on Hitler in the last year or two; but rather than help our understanding of that abominable man, most of them have beclouded rather than clarified his image. A major exception is this impressive volume by Joachim C. Fest. Like Konrad Heiden and Alan Bullock before him, and in contrast to the more recent psychohistorians, Fest is more concerned with "Hitler and his times"—which really should have been the title of his book—than with "Hitler the man." As a human—as distinct from a political—being, Hitler, according to Fest, was an *Unperson*, a man with little personal life, no family, no friends. No anecdotes were ever told about him.

Fest's book has been a runaway bestseller in Germany. This success, together with the fact that the author is a journalist rather than a professional historian, might make one suspicious of the book's quality, especially since it appears at the height of the "Hitler boom." But such suspicion is unjust. Fest is no newcomer to the study of the Third Reich. He first became known ten years ago through a collection of perceptive profiles of Hitler's leading henchmen, *The Face of the Third Reich* (1963, 1970). Later he assisted Albert Speer in the preparation of the latter's memoirs, *Inside the Third Reich* (1969, 1970). His latest book certainly is superior to anything published on Hitler in recent years and as such it has received the generous praise of some of Germany's leading historical scholars.

Aside from telling the ever-fascinating story of Hitler's rise and fall, based on the latest available published, and some unpublished, material, this is basically an interpretive study. To

distinguish between fact and interpretation, the author employs a very effective editorial device, interrupting his main narrative with an occasional and separate *Zwischenbetrachtung*, or "intermediate reflection," on the deeper meaning of the events he has just told. Combining sound knowledge with fruitful intuition, Fest comes up with many suggestive and fresh views on old problems. There are no startling new factual discoveries, and some scholars who have cultivated only a small corner of the vast field of Hitler studies may feel that their findings do not receive their due recognition. Fest, for instance, does not attribute the same significance to certain major events and experiences in Hitler's life that most other writers do. The three events that are generally considered to have been the most traumatic in Hitler's life—his mother's death, his disablement combined with Germany's defeat in 1918, and the suicide of his niece in 1932—in Fest's treatment play only a subsidiary role in the all-important development of Hitler as a political being. Far more significant than any of these personal crises was the abortive putsch of 1923, from which Fest dates the beginnings of Hitler's political growth. Rather than dwell on the often trivial quirks of his subject's personality, Fest has placed him in the context and climate of his times. Basic to that climate was what, in one of his "reflections," Fest calls *die grosse Angst*, the all-pervasive "great fear" that haunted the middle class at the turn of the century and after—fear of revolution, of social and cultural decline, of racial decay, of the rise of the colored races. It was a fear that Hitler shared and which, by playing on its prevalence in others, he used so effectively in his climb to power.

This is such an immensely rich and stimulating book that a short review can hardly do justice to it. It is not flawless and some of the author's sometimes contradictory interpretations and hypotheses will be challenged. Nor is it, of course, the definitive or last word on Hitler—that we shall never have. But it is by far the best we have and will be difficult to surpass.

A word of caution, however, must be added concerning the English edition. It is not a faithful rendition of the German original. It is considerably shorter, its notes have been reduced to more than half, its bibliography has been pared down, and, most regrettably, it fails completely to capture the subtlety and sensitivity of the author's highly individual style. What is brilliant in the original has become drab through translation. The copious illustrations,

on the other hand, while not the same in the two versions, are excellent in both.

HANS W. GATZKE
Yale University

ROBERT R. TAYLOR. *The Word in Stone: The Role of Architecture in the National Socialist Ideology*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xv, 298. \$15.00.

This monograph examines the ideological role claimed for architecture in the Third Reich by cultural nationalists. Professor Taylor utilizes published materials authored by leading and lesser Nazis, party agencies, architects, and "sympathetic writers on culture" to demonstrate the multifaceted ideological role they demanded from architecture. He admirably explains why nationalist and *völkisch* architects—adherents of two amorphous architectural "schools" distinguished by their nationalistic and racist approaches to architecture—rallied to National Socialism, anticipating that mutual ideological and cultural affinities would produce architectural unity. Taylor believes their own ideological imprecision and stylistic relativism, when reinforced by the regime's ambivalent ideological emphasis on architecture, helped ensure that no uniquely "German" architectural style developed after 1933. As he persuasively demonstrates, public architecture remained highly eclectic.

The subsequent ease with which the cultural nationalists adjusted to the architectural reality of the "New Germany" elicits little critical comment from Taylor. He chronicles their satisfied responses to the various functions the government found for architecture, and he records their inevitable "discovery" of the Aryan creativity, racial unity, or national grandeur embodied in such endeavors as *Thingplätze*, bridges, memorials, and *Autobahnen*. Conceding them artistic sincerity, Taylor is unwilling to suggest that cynicism, opportunism, or censorship might account for their critical unanimity. He also fails to question this recently acquired solidarity when indicating their repetitive insistence that, because of its important ideological function, architecture was the premier fine art. One wonders who they convinced besides themselves; Taylor regrettably does not analyze their external influence, nor does he assess the ideological significance that architecture possessed for anyone else in German society.

While very informative in places, innovatively organized, and supplemented with ef-

fective illustrations, the book suffers from limited focus. Concerned primarily to relate how cultural nationalists reacted to what was built, Taylor largely ignores the architectural policies developed by party and state agencies. More analysis of the ideological motivations behind their programs would have increased the book's value. One further defect concerns the biographical data included on the less familiar cultural nationalists who figure prominently in the study. The sketchy background information provided limits their character development, making it difficult to evaluate their ideological or professional significance.

LARRY V. THOMPSON
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MARTIN FRITZ. *German Steel and Swedish Iron Ore, 1939-1945*. (Under Pressure from Abroad: Sectors of the Swedish Economy 1939-1945. Publications of the Institute of Economic History of Gothenburg University, 29.) Gothenburg: the Institute. 1974. Pp. 136. 30 S. kr.

When Martin Fritz questions the role of Swedish iron ore in Hitler's war, his purpose of potential interest is to examine the arguments of Burton Klein and others that German industry was insufficiently mobilized until Albert Speer and 1943. Fritz raises the doubt that steel production could have been significantly increased, first by analyzing the available varieties of ores and their sources. He describes the shortage of blast furnace capacity, which was made more serious by the enforced use of the expensive and inferior German ores resulting from Goering's autarchic Four Year Plan. He notes further shortages of scrap iron, labor, railroad and ship transport, and even of coal energy. War conquest ameliorated some shortages but increased those of skilled labor and shipping. Steel production in the *Altreich* stagnated, but the significant limiting factor was not the supply of ore.

After these provocative observations, the book becomes less rewarding. The treatment of the need for high-quality Swedish ore is belabored, as is the discussion of an unrealistic Allied diplomacy, like Britain overreacting in 1940 by planning to use the Russo-Finnish War as pretext to seize Swedish mines. Allied diplomatic pressure on Sweden accomplished less than the sinking of German ships.

The latter chapters become repetitive. They neither analyze how the iron ore was utilized nor what shortages occurred in war production, either as a result of the limited resources or the

bad planning of Hitler. Fritz only notes in passing the vicious cycle of the lack of ship-building partly because of the lack of steel, which made ore transport more difficult. The book lacks the perspective of the German industry compared to any other steel industry.

Although the author uses German, Swedish, and Allied sources, the resulting analysis does not keep pace. Instead of rising to a comprehensive conclusion, the book declines into repetitions and truisms. The effect is further diminished by a translation, which is adequate for factual presentation but not for subtle distinctions. Scholars may find the statistics on ores useful, and the general reader may be served by the reminder that German economic power was limited by economic and geographic realities as well as by political judgments.

EDWARD N. PETERSON
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River Falls

PETER HÜTTENBERGER. *Nordrhein-Westfalen und die Entstehung seiner parlamentarischen Demokratie*. (Veröffentlichungen der Staatlichen Archive des Landes Nordrhein-Westfalen. Series C: Quellen und Forschungen, volume 1.) Siegburg: Respublica-Verlag. 1973. Pp. xii, 571. DM 49.

With the 1970s post-World War II German history has truly come of age. Peter Hüttenberger's *Habilitationsschrift* is an eminent and most recent case in point. As other recent key works on this period, it is a systematic, tightly knit, primarily political analysis, which on a larger theoretical level aims at an understanding of the evolution of democratic government on the model of Bracher's *Auflösung der Weimarer Republik*.

As archivist in the NRW Hauptarchiv, Düsseldorf, Hüttenberger has been able to assemble an impressive array of primary source material and has built on that a systematic structure of comprehensive historical interpretation. The long glorified "men of the first hour" are effectively demythologized as it becomes clear that old personal and social antagonisms dominated postwar German politics from the start. In a setting of socioeconomic chaos and ever-present—but neither consistent nor always intelligent—British intervention, political apathy and mistrust reigned in the population, and frustration or resignation gripped the native leadership. Noting the conspicuous lack of an "ideological, normative, emotional bond" in pre-cold-war, pre-anti-Communist Germany, Hüttenberger lays bare, but curiously never

actually discusses, a fascinating contrast to the highly politicized, "ideologized" situation in Bracher's dying Weimar Republic. In the absence of any popular legitimization or clear, consistent directives from the military government, the crucial assets of the only successful establishment of democracy in Germany appear to have been, to judge from NRW, the great number of experienced politicians—despite the war—driven by simple political motivations, such as provincial love of homeland, religious or humanistic socialism, and increasingly material interests. In contrast to 1848 and Weimar, German constitution makers felt little need to imitate; their most important lessons were negative ones from their own past.

Hüttenberger presents vital new material for postwar German history generally on the (re)founding of CDU and Center party, the *Landtag* and administration, and on social reform and the 1950 Arnold government—the latter containing fascinating material on Adenauer. The chapter on social reform represents a crucial pioneering effort showing the significance of *résistance*-type forces in Germany and their suffocation first by the military government and later by reawakening economic interests. The chief unfulfilled promise of the book is the disappointing treatment of pressure groups. The author never became altogether comfortable with the socioeconomic side of the picture.

DIETHELM PROWE
Carleton College

NARCISO NADA, editor. *Le relazioni diplomatiche fra l'Austria e il Regno di Sardegna*. Second Series: 1830–1848. Volume 2 (2 luglio 1833–19 luglio 1838). (Fonti per la storia d'Italia. Documenti per la storia delle relazioni diplomatiche fra le grandi Potenze europee e gli Stati italiani, 1814–1860. Part 2, Documenti esteri.) Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1973. Pp. xiv, 557. L. 8,000.

These documents of Austrian provenance demonstrate Charles Albert's growing disaffection with Metternich's policies. The king was voluble in his complaints about Louis Philippe, and his strong support of Don Carlos in Spain placed him in conflict with the maritime powers. Metternich constantly argued that geography and the need for general European peace prevented intervention by the conservative powers, and Charles Albert manifestly disliked the lack of action.

The Austrian chancellor was very much irritated by rumors of a Neapolitan-Sard alliance

against revolutionaries and in effect forbade such an alliance or an Italian federation sponsored by the two states. There were differences over the measures to be taken against the Swiss Confederation, which sheltered refugees who were suspected of plotting descents upon Savoy. By 1836 the Sard ruler indicated he was no longer impressed by talk of a united front to coerce the Swiss. What had his conservative allies done for the king of Holland and Don Carlos?

Both Austrian envoys to Turin, Henri Bombelles and Lazzaro Brunetti, discovered in time that they were distasteful to the king and to Solaro della Margarita, his alter ego at the ministry of foreign affairs, after February 1835. The exchanges between the latter and Brunetti make for quite interesting reading. The next volume will deal with Felix Schwarzenberg's tenure as minister plenipotentiary. It should be even more interesting.

WILLIAM A. JENKS
Washington and Lee University

HEINRICH BULLINGER. *Werke*. Part 1, *Bibliographie*. Volume 1, *Beschreibendes Verzeichnis der gedruckten Werke von Heinrich Bullinger*, edited by JOACHIM STAEDTKE. Part 2, *Briefwechsel*. Volume 1, *Briefe der Jahre 1524–1531*, edited by ULRICH GÄBLER and ENDRE ZSINDELY. With an introduction by FRITZ BÜSSER: *Die Überlieferung von Heinrich Bullingers Briefwechsel*. Zurich: Theologischer Verlag. 1973. Pp. xxiv, 322; 268. 48 fr. S.; 53 fr. S.

Heinrich Bullinger may well have had a greater European influence than any of the reformers except Calvin and may have done more to disseminate Zwinglianism than Zwingli himself, who died in 1531, whereas Bullinger lived to 1575. A complete critical edition of his works and letters is envisaged. These two initial volumes consist, first, of a bibliography arranged in four sections: works Bullinger both wrote and published himself; those on which he collaborated; those he published but did not write; and posthumous editions from his manuscripts. The total number of editions and translations comes to 722. Except Spanish, Portuguese, and the Scandinavian tongues, the translations are into all of the European tongues including Polish, Czech, and Hungarian. Renderings into Arabic and Turkish are known to have existed.

The second of these volumes makes a beginning on the publication of the letters (1524–31) up to the time when Bullinger became Zwingli's successor. The total number of letters bids fair

to reach fifteen thousand. The reason for the approximation is that the libraries of the world have not yet been fully canvassed. The editors wisely decided to start with the thousands already available in Zurich. A supplementary volume no doubt will eventually take care of whatever may come to light in the meantime. Some of the letters have already been published in the correspondence of other reformers such as Calvin, Beza, and Oekolampadius, as well as the English reformers and the Polish. But only a few of these measure up to modern editorial standards. A large number of Bullinger's letters were transcribed by Traugott Schiess. This is no mean contribution because Bullinger's hand is so difficult that his baptismal register, consisting only of names, is said to be illegible. The epistolary category includes letters proper, dedications, and even tracts when addressed to some person. The languages are Latin and Swiss German. Equivalents in modern high German are given in the notes for many of the Swiss forms. This is not superfluous erudition because, apart from dialect, words change their meanings in four centuries. Every reference in the letters is of course identified, but, more than that, every person receives a biography, sometimes of nearly a full page in footnote type. Bullinger says, for example, "My father"—then nearly a page on him. "My mother"—she does not get as much. Women were not so well chronicled.

The subjects of the early letters naturally include discussion of the use of images, the Eucharist, excommunication and the like. Very interesting and rather different are the letters to his fiancée. At the age of twenty-three he had received a verbal promise from a nun. He then wrote her a veritable treatise on harmony in marriage through mutual commitment to the will of God. She stunned him by replying that the will of God bound her to the monastic vow—her mother had told her so. He then submerged her beneath Biblical citations and he won.

ROLAND H. BAINTON
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SERGIO ZOLI. *La Cina e la cultura italiana dal '500 al '700*. Bologna: Pàtron Editore. 1973. Pp. 150. L. 2,200.

Zoli's title promises a study of Chinese influence on Italian culture from 1500 to 1800. What he has given us is three short bibliographic essays on China in sixteenth-century Italian culture, China and the reports of missionaries, mainly

Italian Jesuits, in the seventeenth century, and the Chinese craze in Europe and Italy in the eighteenth century. He notes primary and secondary sources, by no means Italian alone, quoting at length from many. Perhaps half of the book consists of these citations (often very interesting and, happily, more readable than Zoli's text) and of footnotes. Zoli is also generous with quotations from easily available works of Machiavelli, Bodin, and Voltaire and from such modern historians as Eugenio Garin and Federico Chabod. It is difficult to fathom his purpose. Little thematic organization is evident although topics as interesting as the reception of Confucius in Europe and Chinese historical chronology are touched on. Then this odd little book ends. There is no index, a major failing in a work intended, one hopes, more to be used than to be swallowed in one gulp. In his preface Zoli promises a further study of China in the Italian Enlightenment that will explore Italy's role in Europe's eighteenth-century love-hate relationship with things Chinese. While we all wait, the general reader might want to start with Edwin J. Van Kley's "Europe's 'Discovery' of China and the Writing of World History" (*AHR*, 76 [1971]: 358-85). For the rest, Sinologists adept in Italian and scholars interested in traveler's impressions of China or the early Jesuit response to Chinese culture may find something for them in Zoli's sparse text or epic footnotes. Others should know that they are missing little.

GEORGE R. F. BAKER
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GIUSEPPE ARE. *Economia e politica nell'Italia liberale (1890-1915)*. (Saggi, 136.) [Bologna:] Il Mulino. 1974. Pp. 380. L. 5,000.

It is generally agreed that between 1880 and 1918 large-scale capital-using firms came to dominate industrial production in Italy, and in many rural areas capitalist farms pushed out older, less-commercial, and less-economically efficient forms of production. During the same period, the ruling liberal oligarchy began to lose power to mass-supported political parties. This period of transition raised important questions for contemporary Italian economists. Would higher tariffs and greater government intervention in the domestic economy support or retard economic growth? What role should the financial system play in the country's industrial development, and what institutions in particular should be encouraged? Did the rise of a strong working-class movement foster or

limit capitalist development? What part should agriculture play in Italy's development, and what type of agrarian reform would promote capital accumulation within that sector? What industrial structure was most consistent with rapid economic growth—small-scale, perfectly competitive firms or large-scale oligopolistic enterprises? Should emigration be encouraged or discouraged? Were the apparent discontinuities and inadequacies of Italy's development attributed to a lack of natural resources or other "objective" factors, or were they due instead to the educational system, social attitudes, or other features indigenous to Italian culture? How did control of modern technology by more advanced capitalist countries influence the structure and rate of growth of Italian industry?

According to Are, these were the questions that occupied Italian economists from 1890 to 1915, and in the seven essays in this book, all but one of which were published in Italian journals between 1969 and 1972, the author examines how various of the more well-known and politically influential economists of the period treated these issues. As Are correctly emphasizes, such a study is more than an exercise in the history of doctrine because these issues represented real economic problems for the country, and the way they were eventually resolved affected Italy's economic and political development.

The book has a unity and purpose rare for a collection of essays, and it is useful to have the articles available in a single volume. Some parts of Are's analyses will appeal only to specialists in the history of economic doctrine, but for the most part anyone interested in Italian economic history between unification and the rise of fascism will find this a valuable book.

JON S. COHEN
University of Toronto

BETTY BOYD CAROLI. *Italian Repatriation from the United States, 1900-1914*. New York: Center for Migration Studies. 1973. Pp. viii, 110. \$4.95.

In this slender volume students of immigration are reminded that the Atlantic Ocean was a two-way street. Approximately twelve million persons never became—to use sociological idioms—absorbed, integrated, or assimilated into American life. Drawing upon official sources in Italy and the United States and diaries and memoirs left by a few of the repatriates or interviews with others, Betty Boyd Caroli provides twenty-nine tables detailing the

extent of Italian repatriation, as well as statistical characteristics such as age, sex, port of destination, class of travel accommodations, rates of illness or death, and much more.

Perhaps her most interesting chapter discusses the reaction of the Italian government to emigration and repatriation. For the first two-thirds of the century, the states of southern Italy had been officially hostile to emigration, but policy changed with the success of the Italian unification movement: by 1890 officials considered emigration a necessary safety valve for the excess population of the Mezzogiorno.

Within little more than a decade, however, the official *Bollettino dell' Emigrazione* was emphasizing the benefits derived from temporary emigration. Much attention was given to the money coming to Italy from its citizens working in other countries. Between 1902 and 1914 more than \$120 million entered Italy from North America. Since remittances stopped almost entirely when Italians settled permanently in another country, the government encouraged temporary emigration by publicizing its approval of repatriation; by making it easy for "birds of passage" to resume their Italian citizenship; by promoting and subsidizing organizations that fostered ties between emigrants and Italy; and by providing discounts on parts of the return trip. In addition to its comprehensive tables, this informative book has an excellent bibliography but lacks an index.

J. B. DUFF
Seton Hall University

JOSEPH M. KIRSCHBAUM, editor. *Slovakia in the 19th & 20th Centuries: Proceedings of the Conference on Slovakia Held during the General Meeting of the Slovak World Congress on June 17-18, 1971, in Toronto, Ontario, Canada*. Toronto: Slovak World Congress. 1973. Pp. 368.

This is a collection of sixteen papers (some with commentaries) on various aspects of Slovak history in the nineteenth and twentieth centuries. The papers were originally read at a conference of the Slovak World Congress, an organization of Slovak exiles, in Toronto in June 1971.

Slovak historiography has had very special problems to resolve. Until 1918, when it became an administrative unit in the newly founded Czechoslovak Republic, Slovakia was, for a thousand years, an integral part of the kingdom of Hungary. While Slovakia was a new political entity in 1918, the Slovaks were not a new people. In fact, they had emerged on the stage

of European history, together with the rest of the Slavs, well before the formation of the Hungarian state in the tenth century. Unlike the Croats, however, they were incorporated into Hungary outright, becoming one of its "submerged" nationalities. They had no province or territorial unit with which to identify themselves. Until the rise of modern nationalism, this scarcely mattered, because Hungary was (legally until 1848) a corporate state in which the population was organized into feudal estates without regard for its ethnic divisions. Latin was Hungary's official language as well as the language of intellectual intercourse in the country.

At the end of the eighteenth century, partly under the influence of the Enlightenment and partly as a reaction to the centralizing and Germanizing reforms of Emperor Joseph II, Hungary's nationalities awakened to a modern sense of nationalism. They discarded Latin and elevated their respective vernaculars to the dignity of their literary tongues. For the Slovaks, this task was performed by Father Anton Bernolák (1762–1813), a Catholic priest and scholar. His linguistic reform, however, encountered the opposition of the Slovak Protestant minority. Since the sixteenth century when Protestantism was introduced into Slovakia, partly by preachers from Bohemia, the Slovak Protestants used the Czech language for liturgical purposes. Now they began to use it also as the Slovak literary tongue. In the light of Czech and Slovak linguistic proximity, the Slovak Protestant intellectuals Jan Kollár (1793–1852) and Pavol Jozef Šafárik (1795–1861) leaped to the conclusion that the Czechs and Slovaks were one and the same people—despite their manifest historical, social, and cultural differences. L'udovít Štúr (1815–51), another Slovak Protestant intellectual and the foremost ideologist of Slovak nationalism in the nineteenth century, promoted another Slovak literary tongue that won the acceptance of both religious communities in Slovakia. This healed the temporary linguistic schism between the Slovak Catholics and Protestants but created a permanent one between the Czechs and Slovaks as a whole.

Even after the abandonment of the Czech language by the Slovaks, the doctrine of Czechoslovak unity continued to receive the support of some Czech and Slovak intellectuals, among them Thomas G. Masaryk (1850–1937), the founder of Czechoslovakia. In World War I, believing the Slovaks to be simply "unredeemed" Czechs in Hungary, like the unredeemed Ital-

ians in Austria, he made it a point of his program to liberate them and include them in the Czechoslovak state. He succeeded in his aim, and the doctrine of Czechoslovak unity was given official expression in the constitution of the First Czechoslovak Republic. It encountered the opposition of the rising generation of Slovak nationalists, however, and was officially discarded in 1945. The difficult conceptual problems Slovak historians had to resolve before undertaking normal tasks of their craft are perhaps evident from the foregoing. They had to define what Slovakia was and who the Slovaks were—indeed, to determine whether the Slovaks were a distinct people at all.

As is inevitable in a symposium, the essays in this book greatly vary in method and quality. Some of them are products of conventional scholarship and are equipped with references to sources in footnotes, while others are reflective in character and lack of scholarly apparatus. Whatever their character, most of them give new insight into the problems of Slovak history. The volume contains a bibliography but lacks an index, which is a particularly regrettable deficiency in a symposium. Despite this and other shortcomings, this volume will undoubtedly be received as a welcome contribution to the limited Slovak historiography in the English language.

VICTOR S. MAMATEY
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PHILIP GABRIEL EIDELBERG. *The Great Rumanian Peasant Revolt of 1907: Origins of a Modern Jacquerie*. (Studies of the Institute on East Central Europe, Columbia University, New York.) Leiden: E. J. Brill. 1974. Pp. xii, 259. 64 gls.

Subjecting an immense quantity of archival and published material to rigorous critical analysis, the author has succeeded in synthesizing, as no one before him has done, the underlying and immediate causes of one of the major peasant revolts in East European history. He is thoroughly acquainted with the political and agrarian history of Romania in the half-century before the First World War, and, although his main purpose is to discover the causes of the revolt of 1907, his survey of the period is so extensive that he goes far toward filling the gap in Western scholarly literature between Marcel Emerit's *Les paysans roumains depuis le traité d'Adrinople jusqu'à la liberation des terres* (1937) and Henry L. Roberts's *Rumania: Political Problems of an Agrarian State* (1951).

Unlike earlier historians of the revolt, both Romanian and foreign, Eidelberg traces its origins not to the agrarian reform of 1864, which he characterizes as the final product of an earlier era, but to the first drastic decline in world grain prices in 1875. Unlike his predecessors also, he devotes special attention to two districts in northern Moldavia where the first uprisings occurred in February 1907.

The author examines in some detail those aspects of Romanian agrarian development since 1875 that exacerbated the condition of the peasantry and eventually led to violence. He finds that the system of agricultural contracts, which generally provided for rent payments in labor rather than money, was the chief cause of peasant unrest. It, in turn, was closely linked to the widespread practice of the great landlords of leasing part or all of their estates to middlemen (*arendăși*), who looked upon the peasants as a source of income and, accordingly, drove hard bargains with them at contract time. Proposals for reform came from many directions, and Eidelberg's focus on the Liberal party and Vasile Kogălniceanu, the leaders of the Moldavian small landholders, constitutes an original approach to the causes of the revolt of 1907. Eidelberg shows how the Liberals promoted the Popular Bank Law of 1903 and the Village Cooperative Law of 1904 not to save the peasantry as a whole, but rather to encourage the growth of a native industry. His analysis of Kogălniceanu's radical reforms goes beyond the immediate economic issues to include the question of Moldavian autonomy within the Romanian kingdom and relations with Austria-Hungary. He classifies Kogălniceanu as a conservative populist who wanted an agrarian reform to aid the peasantry but who, at the same time, opposed the village co-operative movement on the grounds that it aimed to destroy the entire landlord class. The final chapter, a detailed analysis of the immediate causes of the revolt in northern Moldavia, brings together all the different strands of the long-term crisis described in earlier chapters. Eidelberg concludes that peasant unrest was caused largely by other classes, and, consequently, he finds the term "peasant revolt" misleading.

This book, painstakingly argued and scrupulously documented, merits the epithet original. It has much to tell us not only about 1907 in Romania, but about peasant revolts in general.

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JANINA JANOWSKA *et al.*, editors. *Archiwum polityczne Ignacego Paderewskiego* [Political Archives of Ignace Paderewski]. In four volumes. Wrocław: Zakład Narodowy im. Ossolińskich. 1973-74. Pp. 596; 700; 262; 419. Zł. 750.00.

With musical eloquence Ignace Jan Paderewski won the attention of the world, then with impassioned oratory he pleaded the cause of Poland before his captivated audiences. Of India's Gandhi he asked: "On behalf of a nation who is today defending the sacred right to remain free against a cruel and nameless tyranny I appeal to you as one of the greatest moral authorities of the world to use your noble influence with your countrymen to gain for Poland their sympathy and friendship." To Britain's Lloyd George, to the United States's great statesman Woodrow Wilson, to the leaders of France, to people the world over, Paderewski went and presented his case—the cause of Poland. He wanted more than anything else a united, independent, and autonomous Poland at a time when his partitioned homeland was not even on the maps of Europe. Relying on his musical virtuosity, his abundant energy, and a finely honed sense of justice, Paderewski became Poland's roving ambassador, entralling audiences everywhere with his mastery of the piano and politics.

Paderewski's activities are reflected in the tens of thousands of his letters and other papers, which are now housed mostly in Poland's national archives. In 1973, thirty-two years after Paderewski's death, much of his political correspondence was published for the first time in a four-volume collection under the aegis of the Polish Academy of Sciences.

The more than 1,400 documents in these volumes were selected primarily for their relevance to the political and social problems of the Polish people both at home and abroad. All the papers and letters are reprinted in their original language, and an index of names and places is included in each volume. While the volumes are an important study of Paderewski's Poland, the collection also documents the extensive efforts of the United States government and Polish-American organizations to aid Poland in gaining independence.

The first volume of the series covers the years 1890-1918, a period when Poland was openly struggling to regain autonomy. Paderewski worked actively for the Polish National Committee in Paris, which later was recognized by the United States as the official representative of Polish people throughout the world. The

musician also helped recruit a Polish army. United States citizens of Polish descent formed the nucleus of the army, which eventually numbered about 100,000.

The second volume contains correspondence written during Paderewski's term of office as Poland's prime minister (January–December 1919). Correspondence from the immediate post-war period (1919–21) provides insight into Paderewski's increasingly deeper involvement with the foreign policy (including boundary problems) of his reborn republic and illustrates his advocacy of Poland's case before the League of Nations—from July 1920 to April 1921 Paderewski served as Poland's official spokesman at international congresses and meetings.

The third volume spans the years 1921–34 when Paderewski's official political activity waned and his artistic endeavors increased. After a coup d'état in 1926, Paderewski opposed the regime in Poland.

By 1935, Paderewski had returned to the political scene, and the fourth volume is composed of his papers dating from this time to 1940. The publication does not include correspondence from September 1940 to June 1941, when he died in New York at the age of eighty-one. He had just finished a series of speeches to Polish people fighting for the survival of their country and to the American people on behalf of his enslaved nation.

Paderewski worked relentlessly for Poland, representing his country skillfully and articulately. Former U.S. Secretary of State Robert Lansing later wrote about these efforts: "When the famous musician came to see me at my office at the Department of State, I could not avoid the thought that his emotions were leading him into a path which he was wholly unsuited to follow. In truth, I thought he was making a mistake. . . . My second impression—and it is the impression that I still hold—was that I.P. was a greater statesman than he was a musician."

Another United States official who befriended the Polish patriot was Colonel Edward House, adviser to President Wilson. Calling House "Poland's providential man," Paderewski discussed the Polish situation with him at length. Because of Paderewski's diligence and House's intervention, Wilson, in his famous Fourteen Points, called for a "united, independent, and autonomous Poland."

While the editorial work is not without some flaws of which the investigators should be aware, these four volumes of Paderewski's correspondence will be highly useful to scholars and re-

searchers seeking a better view of this talented, tireless statesman. The compilation also documents an insider's assessment and participation in a most important period of Poland's modern history.

JANINA HOSKINS

Library of Congress

JAN. M. CIECHANOWSKI. *The Warsaw Rising of 1944*. (Soviet and East European Studies.) New York: Cambridge University Press. 1974. Pp. xi, 332. \$19.50.

Mr. Ciechanowski's book stems from a doctoral dissertation written for the University of London and is a translation of a Polish version published in London in 1971. It is the first detailed study of the Warsaw Rising of 1944 to be published in English and is based on extensive Polish archival sources deposited in London, as well as on oral evidence collected from key participants. It is a most welcome and, indeed, crucial contribution to our knowledge of the complex and much misunderstood subject of the Polish question in the final years of the Second World War.

The author painstakingly documents both the evolution of the problem of Polish-Soviet relations in the diplomacy of the three great powers and the development of the political and military views on the part of the Polish government in London and the underground authorities in Poland. The study is informed throughout by Ciechanowski's conviction that the only realistic policy for both sets of Polish authorities was to have accepted Stalin's demands—that is, the establishment of the Polish-Soviet frontier on the Curzon Line and the reconstruction of the London government by eliminating those members who were opposed to such an agreement. He contends that only such a settlement could have led to the establishment of a non-Communist government in Poland, at least for a while.

One cannot quarrel with this judgment. The question is, however, whether such a policy could have been implemented by the non-Communist Polish leaders at the time. On the basis of the documents cited, it is clear that the solution proposed was unacceptable to most Poles, both in London and in Poland. Certainly the leaders and lower cadres of the underground were convinced in the light of Polish history, particularly the most recent, which had witnessed German-Soviet collaboration against Poland in 1939–41, not to speak of the Katyń affair of 1943, that Soviet Russian demands for prewar Polish territories were only a prelude to

the sovietization of Poland. They counted on the swift development of disagreements between the Western Allies and the Soviet Union, particularly in the race for Germany, which they expected to take place rapidly after the German collapse, which, in turn, they thought would occur in the summer of 1944. All these calculations proved to be premature, as was the outbreak of the Warsaw Rising itself. Nonetheless, this was the human element that was involved, and it is unrealistic to blame people for being what they are, however much we may disagree with their assumptions and beliefs.

It seems as if there was one possibility of avoiding the tragedy of 1944. Mikołajczyk was willing to agree to the establishment of a "demarcation line," giving *de facto* administration of the eastern territories to the Soviet Union, while keeping the *de jure* settlement for the immediate postwar period. Stalin, however, refused to entertain Churchill's proposals to this effect in the spring of 1944. Mikołajczyk's last card in these circumstances was the Warsaw Rising, which was to document both the anti-German activity of the Home Army, loyal to London, and to allow the establishment of a non-Communist administration in the Polish capital a few hours before the expected entry of the Red Army. But the rising broke out prematurely. While the Soviet forces were unable to help in the month of August, they made no serious effort in September either. Stalin also refused to allow the landing of Allied supply planes behind the Soviet lines.

Many of the key documents cited by the author, particularly the Churchill-Mikołajczyk conversations in early 1944, are available in English in the *Documents on Polish-Soviet Relations, 1939-1945*, published in London in 1961-69. It would have been helpful if Ciechanowski had indicated this, but he usually cites them as archival materials. For some reason no diacritical marks are used in the book, although this is the customary usage in scholarly publications. Finally, the organization of the book, which is thematic rather than chronological, tends to obscure the development of Polish strategy in London and in Warsaw. These minor criticisms do not detract from the overall value of the study.

ANNA M. CIENCIALA
University of Kansas

G. P. MESHCHERIAKOV. *Russkaia voennaia mysl' v XIX v.* [Russian Military Thought in the 19th Century]. (Akademiia Nauk SSSR, Institut

Voennoi Istorii, Ministerstva Oborony SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 314.

The Russian army in 1801 represented the old regime, but it had progressive traditions from the Suvorov era. The defeat at Friedland, however, led to reforms, which brought in young, liberal officers. After Napoleon had fallen, however, reaction set in, which resulted in the Decembrist uprising in 1825. The author, who is at the Institute of Military History, uses an abundance of sources to recount the struggle between the reactionaries and the modernizers. Although the Petrashevtsy came to grief, even under Nicholas I, D. A. Miliutin could come to the fore.

After the Crimean defeat came the Great Reforms, with Miliutin as minister of war transforming the army organization, aided by the revolutionary N. G. Chernyshevski, an editor of *Voennyi Sbornik*. Although the latter was soon exiled to Siberia, Miliutin, thanks to the development of Russian industry and railways, was able to change the whole military system and to introduce universal service. He also encouraged the officers to study military science and to learn from Russia's historical experience.

When Miliutin resigned in the reaction after Alexander II, his work still went on. Russian officers provided modern artillery and rifles and learned to use railway transport. In tactics, open order replaced columns and skirmishers, and the cavalry was trained to fight on foot. There was much analytical study of Russia's wars. But the old guard did not surrender, and even Generals Leer and Dragomirov, leading military thinkers, still held that the bayonet rather than the bullet won battles. On the other hand, intensive studies were made of provisioning armies in the field, and the army adopted an excellent magazine rifle. The study of Russian military history stressed the principles of warfare rather than the glory of the commanders. Thus the Russians proved they were capable of advanced military science, but the leaders around the throne opposed innovations. Only when Russia had a more understanding regime would its military thinking come into its own.

G. P. Meshcheriakov has produced a sound piece of scholarly historical research.

JOHN SHELTON CURTISS
Duke University

JOHN L. EVANS. *The Petraševskij Circle, 1845-1849*. (Slavistic Printings and Reprintings, 299.) The Hague: Mouton. 1974. Pp. 114. 32 pls.

Surely it is very hard to make a *prima-facie* case that a book is unnecessary. There is always room for another re-examination of an issue, particularly if it crosses a language barrier. We would like an English synthesis on Petrashevsky and his group that would bring together the pre-Revolutionary work of V. I. Semevsky, the documents published later about the investigation and the texts of their writings, and the analysis of Soviet historians such as V. R. Leikina. One does not need "work in the archives" to justify such a project.

Unfortunately, this book does not do the job. It summarizes, in a very sketchy form, previous work without adding to it. There is no attempt to distinguish what is unique in the thoughts of the Petrashevtsy about, for instance, "man's inhumanity to man" or the "nefarious miscreancy" of serfdom from anyone else's commonplace, at any other time or place. The book does not communicate a feeling of ease with nineteenth-century Russian intellectual history; the discussion of Apollon Grigorev (misunderstood out of context from Pushkarev's survey) and of Nikolai Danilevsky (admittedly peripheral to the point) is embarrassingly simplistic, as is the short discussion of serf reform in Russia. Mr. Evans is not to blame for being unimpressed by Petrashevsky's views on social change, but the general nineteenth-century intellectual context is very exciting indeed. On the other hand, one should not merely footnote Feuerbach when discussing Feliks Tol's exposition of Feuerbach's ideas to the Petrashevsky circle; more commentary is necessary.

The best section is that dealing with the details of the circle: where they met, what they ate, how they organized their time. On the other hand, the style has a propensity for understatement, which can merely be bland but sometimes quite humorless, thus defeating the purpose: "Their rancor at discovering who the spy was later was exemplified in the *Petrashkevets* Petr Ivanovich Beletskij, who upon his release less than three months after his arrest, met Antonelli [the spy] on the street and punched him" (p. 96).

LINDA GERSTEIN
Haverford College

V. G. CHERNUKHA. *Krest'ianskii vopros v pravitel'stvennoi politike Rossii (60-70 gody XIX v.)* [The Peasant Question in Russian Government Policy (1860s-1870s)]. (Akademiia Nauk SSSR, Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka." 1972. Pp. 224.

The all too familiar justification for a monograph—the argument that the subject under study has been at least relatively neglected—is in the present case quite defensible. The author convincingly shows that neglect of government policy on the peasant question in the first two decades following the emancipation of the serfs in 1861 has concealed from historians elements of continuity in the development of the state's peasant policies during the final decades of the old regime. It has caused them to interpret spates of agrarian legislation, such as those that occurred in the early 1880s or following the Revolution of 1905, as a species of bureaucratic knee-jerk reaction to short-term situations of stress, whereas in fact "revolutionary situations" only served to precipitate action on legislative projects long in the works; they influenced the character of the legislation rather little.

It is demonstrated quite clearly that people in high places in government did not have to wait for a nationwide famine or the degeneration of the countryside into a general state of anarchy in order to get the point that all was not well in the postemancipation village and to begin generating projects not only on tax reform, agricultural credit, and the like, but also on peasant migration and the destruction of communal tenure and its replacement by household tenure—the proverbial "wager on the strong." This kind of activity was well under way by the late 1860s and was given great impetus in the extensive work of the Valuev Commission (1872-73). Why it resulted in no major modifications of the 1861 legislation during the reign of Alexander II is the central story of the book; it is a story of competing bureaucratic interests and personalities, all played out against a background of relentless fiscal pressures and apprehension about political control, which I will not attempt to summarize here. Suffice it to say that it is told in the straightforward manner, with heavy reliance on bureaucratic archives and the personal papers of statesmen and bureaucrats, generally associated with the school of P. A. Zaionchkovskii. In only a few cases does the author succumb to the vice of "citationism," as, for example, on page 141 where she tries to explain differing views among the landed gentry on the issue of communal versus household tenure in terms of a clash between *rentier* and entrepreneurial interests, but she produces no evidence in support of this explanation except an appropriate quotation from Lenin.

The book might be described as three distinct but related monographs: the first is devoted to government policy in relation to the peace mediators, the special officials charged with implementing the emancipation legislation; the second is devoted to government direct-taxation policies; and the third investigates government policy on the rural commune (*sel'skaia obshchina*). The first is a somewhat revised version of an article published in 1967. Of the three, the third is the most original; so far as I know, the question has never been studied systematically before.

TERENCE EMMONS
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CORRECTION: An unfortunate printer's error survived to appear in Professor Linda Gerstein's review of Allen Sinel, *The Classroom and the Chancellery: State Educational Reform in Russia under Count Dmitry Tolstoi*, which appeared in the April issue of the *AHR*, page 439. The passage at the beginning of the third paragraph of the review should have read: "Sinel's book is not a rehabilitation of Tolstoi. The author never allows himself to fall prey to the rationalizations in the vocabulary of Tolstoi and his associates, and he never loses sight of the political motivations. But he goes beyond the superficial hostility of the liberal critics and examines before he indicts." The omission of the word "never" in the second sentence not only made that sentence nonsensical, but appeared to compromise what Professor Gerstein in a subsequent letter called the book's strongest virtue—"its hardheaded clarity." The editors regret any confusion this compounded error may have caused to readers and the injustice it certainly visited upon Professors Sinel and Gerstein.

K. A. KRIVOSHEIN. *A. V. Krivoshein (1857-1921 g.): Ego znachenie v istorii Rossii nachala XX veka* [A. V. Krivoshein (1857-1921): His Role in the History of Russia at the Beginning of the 20th Century]. Paris: Maison du Livre Étranger; distrib. by N. Martianoff Bookstore, New York. 1973. Pp. 350. 35 fr.

Considerable historical research has been done on prominent twentieth-century tsarist officials. K. A. Krivoshein, however, has filled a notable gap with this biography of his father. Aleksandr Vasilevich Krivoshein completed law school and began government service in the ministry of justice. His marriage and early career enabled him to establish valuable contacts with influen-

tial Moscow industrialists. A man of great ambition and erudition, Krivoshein, nevertheless, regarded his lack of oratorical skills and fluency in foreign languages as serious impediments to government advancement.

Krivoshein left his greatest imprint in agriculture. As minister of agriculture (1908-15) he was responsible for implementing Stolypin's land reforms. In 1906 he created the land settlement commissions that were used to resolve the complex land transfers. Later he repeatedly clashed with Finance Minister Kokovtsov over the request for larger credits to develop irrigation and promote agriculture in Central Asia and the Transcaucasus. Krivoshein vigorously promoted the settlement of Siberia; about 2.5 million peasants settled there during his tenure as minister of agriculture.

During 1913 and 1914 Krivoshein attained his zenith by gradually displacing Kokovtsov. Although he refused the post of chairman of the council of ministers, he was widely recognized as the minister having the easiest access to and greatest influence on Nicholas II. His policy of government accommodation with the Duma and opposition to Nicholas's assumption of military command led to his resignation in October 1915. For the remainder of the war he served in the Red Cross. His last major assignment was during the civil war with General Wrangel in the Crimea where he introduced land reform and *volost' zemstvos*. Krivoshein died in Berlin in 1921.

Krivoshein left few personal papers. Consequently, this study is based primarily on meticulous documentation from Western and Soviet primary and secondary materials rather than family recollections. Aside from the tendency to eulogize and the cursory treatment given Krivoshein's life after his 1915 ministerial resignation, this book truly enhances our knowledge of one of tsarist Russia's last devoted and capable government officials.

JAMES LONG
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L. S. GAPONENKO, editor. *Oktiabr'skaia revoliutsiia i armia, 25 oktiabria 1917 g.-mart 1918 g.: Sbornik dokumentov* [The October Revolution and the Army, 25 October 1917-March 1918: Collected Documents]. (Akademiia Nauk SSSR, Institut Istorii SSSR, Institut Voennoi Istorii, Ministerstva Oborony SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 453.

In 1917 a goal of Bolshevik policy was to undermine discipline in the Russian army. Of course, after their victory the Bolsheviks faced a dif-

ferent and more difficult task. While completing the dismantling of the old army, which had ceased to be an instrument for war, they had to create the rudiments of a new military organization for defense against their foreign and domestic enemies. *The October Revolution and the Army*, a new collection of documents dating from October 1917 to March 1918, is devoted to the crucial period of transformation from the old army to the new.

This volume is the third in a series. The first volume dealt with the history of the revolutionary movement in the army during the First World War, and the second with the revolutionary movement in the army during 1917. The principles of organization, the scholarly apparatus, and the strengths and weaknesses of the three volumes are the same. Each book includes rather inadequate notes and indexes of persons and military units. As in the previous collections, the reader is inundated with a mass of meaningless and extremely repetitious documents, such as resolutions of regimental meetings calling for the support of the new regime in Petrograd, exhortations of party leaders about the dangers of counterrevolution, and communications between officers in which they deplore Bolshevik strength in the army.

It is hardly necessary to say that the uninitiated reader who has the patience to get through the impossibly dull material will not be rewarded for his efforts with a balanced view of the army. Since the editors chose to include only that material which documents Communist strength, it is hard to appreciate the magnitude of the task that Lenin and his comrades faced in creating the Red Army.

Yet, in spite of the tendentiousness of the collection, in spite of the extreme repetitiousness of the documents, the scholar of the period will have to struggle through the volume in hopes of finding a few nuggets of interesting detail. For example, the correspondence reproduced here among senior officers of the old army once again documents how important a role Lenin's initiation of peace talks with the Germans played in the decision of former tsarist officers to take up arms against the Bolshevik regime.

PETER KENEZ
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ZVI Y. GITELMAN. *Jewish Nationality and Soviet Politics: The Jewish Sections of the CPSU, 1917-1930*. (Written under the auspices of the Research Institute on Communist Affairs, Co-

lumbia University.) Princeton: Princeton University Press. 1972. Pp. xii, 573. \$20.00.

In this well-researched book, based on a careful sifting of evidence drawn from archival, periodical, and published materials available in the West, the author attempts to analyze the process of modernization of Soviet Jewry against the background of Jewish conditions in Russia under the tsars. In his approach the author uses the conceptual framework of modernization, political, integration, and social mobilization developed by David Apter, Karl Deutsch, S. N. Eisenstadt, and Amitai Etzioni. Having presented the genesis of the views and the role of Jewish leadership during the 1917-21 period, Gitelman then concentrates on the evaluation of the programs and policies advocated by the Jewish section of the Communist party during the 1920s, before Stalinist controls were effectively introduced.

Considerable effort is expended on a detailed exposition and analysis of organizational platforms and factional squabbles, which illustrate well the process of crystallization of views among the leaders of the Jewish groups. The study stresses the multiplicity of goals among these groups, all of which were aiming to retain distinct national identity but at the same time were also interested in employing the changes introduced in Russia in 1917 and 1921 to benefit the Jewish population. It develops the precepts under which Jewish national life was supposed to be transformed during the 1920s, focuses on the activities of the Jewish sections engaged in this modernizing process, discusses the problems that they encountered in various parts of the Soviet Union, and perceptively explains the reasons for their respective successes and failures by 1930. In the author's view, best results were obtained in the areas of economic opportunity and improvement at the price, however, of diminution of maintenance of distinct Jewish cultural and spiritual values. The picture that emerges from this able treatment of a difficult and complex topic is that of a select elite of the Jewish party and government activists attempting forcefully to remold a largely indifferent and hostile mass of Jewish population in accordance to the new Soviet-socialist conceptions.

Until Soviet archives are opened, this work is likely to remain a definitive treatment of the subject. Omissions and minor errors of fact, for example, in the biographical data of some of the Jewish leaders, do not substantially detract from its intrinsic value. The book not only provides valuable insights into the Communist

party's policy toward the Jewish minority during this period, but it also illuminates the regime's outlook and behavior toward other Soviet national groups. It constitutes an important and welcome addition to the body of scholarly literature dealing with the history and problems of Soviet nationalities.

BENEDICT V. MACIUIKA
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PAVEL V. MYKHAILYNA. *Mista Ukraïny v period feodalizmu (Do pytannia pro stanovyshche mist v umovakh inozemnoho ponevolennia v kintsi XVI-pershii polovyni XVII st.)* [The Cities of the Ukraine in the Period of Feudalism: Concerning the Question of the Situation of the Cities in the Conditions of Foreign Enslavement from the End of the 16th through the First Half of the 17th Century]. (Ministerstvo Vyshchoi i Seredn'oi Spetsial'noi Osvity URSR.) Chernovtsy: Chernivets'kyi Derzhavnyi Universytet. 1971. Pp. 112.

The subtitle of this little book is more indicative of its coverage than the principal title. The work is not, in any comprehensive sense, a study of urbanism in the sixteenth- and seventeenth-century Ukraine; rather, it is an examination of the impact of Polish rule upon the nonrural native elements and foreign merchants. A certain amount of attention is given to the mutual relations of Russian and Ukrainian merchants and their common suffering at the hands of the "foreign" Poles, but the theme of Russo-Ukrainian solidarity is not excessively emphasized, and the oppression of other merchants (for example, the Moldavians) is noted. More significant in terms of general Soviet interpretation of Ukrainian history is the emphasis on the subservience of most urban settlements to the great Polish magnates. As far as I am able to judge, the facts are accurate and representative. In contrast to Western European cities at an earlier stage of development, or even some Russian cities, Ukrainian towns were unable to gain any considerable measure of autonomy from local territorial lords. Nominal urban rights and privileges (derived in several important cities like Kiev, Lvov, and Kamenets-Podolski from Magdeburg law) were flouted. In the cities just mentioned, encroachments by the Crown were most significant, but in general the weakness of the Polish kings prevented the kind of urban-royal alliance that led to urban autonomy in Western Europe. Thus Mykhailyna is undoubtedly correct in characterizing the condi-

tions of the embryonic Ukrainian urban sector as anarchical—tragically, for the future of the Ukrainian nation this anarchy precluded development of a native bourgeoisie. Whether these conditions constituted feudalism or a colonial regime is more debatable.

To me what Mykhailyna leaves out is more significant than the accuracy of his categories. One, like myself, who is no expert on the period can nevertheless sympathize with his regrets concerning the paucity of sources. Still, he throws out enough hints about the native urban patriciate and its monopoly of the mayoral office to suggest that more could have been done to examine the fascinating though ultimately abortive development of this element. It is hard to believe, too, that more could not have been done to distinguish significant categories of urban settlements. Thus Mykhailyna notes that of 323 "cities and little towns" in the Kiev and Bratislava *voevodstvos*, eighty-one per cent were private property of nobles, sixteen per cent royal, and ten per cent ecclesiastical, while in Volhynian *voevodstvo* ninety-six per cent (of sixty-eight) were private. Failure to examine the size of places in these categories leaves even the author's argument for the predominance of private rule unsupported.

JOHN A. ARMSTRONG
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I. K. VASIUTA. *Selians'kyi rukh na Zakhidniy Ukraïni v 1919-1939 rr.* [The Peasant Movement in the Western Ukraine, 1919-1939]. Lvov: Vydavnytstvo L'vivskoho Universytetu. 1971. Pp. 220.

Professor Vasiuta's monograph falls short of his stated goal—to produce a more complete story than hitherto told of the peasant movement in Western Ukraine during the period of Polish occupation from 1919 to 1939. Instead his study is a depressing attempt to expose all the non-Communist political parties and organizations of Western Ukraine and their leaders as betrayers of the national interests of the Ukrainian people and to canonize the Ukrainian Communist party for its unique loyalty to the Ukrainian cause. Vasiuta's work is another indication that the Communist approach to knowledge is unchanged, that the principle of intellectual obedience has not lost hold since Lenin's time.

He attacks, for instance, the independent Ukrainian non-Communist parties for their

"falsely nationalistic phrases about 'sovereign Ukraine'" (p. 34) while expounding upon the national interest of the Communist party of Eastern Galicia (changed to the Communist party of Western Ukraine in 1923); this, despite the fact that the political and organizational leadership over the party was vested in the Communist Workers' party of Poland by an agreement signed in Moscow on April 12, 1921. In other words, whether in Polish-occupied Western Ukraine or in Soviet Ukraine, the Ukrainian Communist parties were territorial units of the Russian Communist party (Bolshevik) and were guided, as Vasiuta himself reminds the reader (p. 29), by directions Lenin had set, directions that obviously were not dominated by the national interests of the Ukrainian people.

Each of the monograph's four chapters covers a five-year period. This chronological approach lends itself to a repetitive discussion, in each successive chapter, of peasants burning estates, striking, boycotting, and so forth. The study is extensively footnoted with predominantly Soviet sources. It lacks, however, an index and a bibliography, both of which would have been helpful. The abbreviations for several of the state and party archives, for example, are not fully clarified in the footnotes or in the author's introduction.

Despite its serious defects, Vasiuta's study shows obvious familiarity with the spontaneous local peasant economic disturbances in Western Ukraine between the two World Wars and with the Communist attempts to organize these into a mass revolutionary movement. It represents, unfortunately, a party-line approach to historical research.

BOHDAN P. PROCKO
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NEAR EAST

WALTER F. WEIKER. *Political Tutelage and Democracy in Turkey: The Free Party and Its Aftermath*. (Social, Economic and Political Studies of the Middle East, volume 8.) Leiden: E. J. Brill. 1973. Pp. xii, 317. 84 gls.

Turkey has been more successful than most "developing" nations in making the transition from an authoritarian to a democratic political system. In this study, growing out of his doctoral dissertation at Princeton University, Wal-

ter Weiker contends that political "tutelage" during the single-party period from 1923 to 1946 was beneficial to both modernization and democracy in Turkey. The focal point is the brief Free party episode of 1930, the abortive attempt by Atatürk to create a "loyal opposition" as "the culmination of his Revolution" (p. 5).

Part 1 summarizes the Atatürk revolution to 1930 and recapitulates earlier Turkish experience with multiparty politics. Part 2 dwells upon the details of the short-lived Free party of 1930 and, in my opinion, constitutes the most substantial portion of the volume. The effects of the Free party experiment upon the "tutelary" role of the ruling Republican Peoples party between 1930 and 1946, including the reorganization of the party, development of the Kemalist ideology, and mass political education, are treated in part 3. Finally in part 4, the author compares the successful emergence of a multiparty system after 1946 with its failure in 1930 and makes a final assessment of the role tutelage played in that success. He ends on the disturbing note that "tutoring a nation for democracy seems to be a far different—and perhaps far easier—thing than tutoring it to be able and willing to forge the many new social, economic and political connections which the modern age demands" (p. 303).

This is one of those books which stimulates and irritates, enlightens and frustrates. The Free party episode does indeed occupy a pivotal position in the political development of modern Turkey, and Professor Weiker deserves our thanks for his thorough study of it as well as for his broader analysis of the relationship between political tutelage and democracy in Turkey. The question might be raised, however, whether the author has not exaggerated the effects of the Free party upon subsequent developments. Even so, my overall impression would have been vastly more positive had the volume not suffered from innumerable and, in most cases, inexcusable minor errors and technical defects. How could the author have allowed the name of one sultan to appear in five variant spellings (pp. 11, 36, 38, 79), have used correctly the Turkish dotted *i* and undotted *i* in lower case but only the undotted form in capital, have overlooked minor errors in quotations and footnote citations (for example, p. 15, n. 1; p. 36, n. 1), and have permitted typographical errors too glaring and numerous to be missed? It is an interesting and informative book marred by carelessness in

some minor details and a seriously flawed production.

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MAJID KHADDURI. *Arab Contemporaries: The Role of Personalities in Politics*. Baltimore: Johns Hopkins University Press, 1973. Pp. x, 255. \$8.95.

In 1970 Professor Khadduri published an important analysis of the process of political modernization in the Arab world, *Political Trends in the Arab World: The Role of Ideas and Ideals in Politics*. His newest work, *Arab Contemporaries*, is a worthy sequel and companion to the earlier volume.

The book treats twelve representative political figures who have played significant roles in Arab politics since World War I. It describes how each of the twelve became involved in politics, the process by which they formulated their political goals and styles, and the actual methods they used as they attempted to realize their aims. Personalities who exemplify three avenues of political leadership are considered: military politicians ('Aziz 'Ali al-Misri, Nuri al-Sa'id, Jamal 'Abd al-Nasir); professional politicians (the Grand Mufti Hajj Amin al-Husayni, King Faysal of Saudi Arabia, Habib Bourguiba, Kamil al-Chadirchi, Kamal Junblat, Khalid Bakdash); intellectual politicians (Ahmad Lufti al-Sayyid, Muhammad Husayn Haykal, Michel Aflaq). Moreover, although the author admits that these men defy neat categorization, he identifies their particular approach to political activity as representing either that of the idealist, the realist, or the ideologist. The authority, value, flavor, and interest of the book owes much to Professor Khadduri's informed personal acquaintance with each of the leaders whose political personality he analyzes.

In a thoughtful final chapter, Khadduri muses on the role of "the great man" in history. He concludes that it is the confluence of the particular qualifications of an individual with the right circumstances that allows a person to play a major role in politics: "in other words, it is neither the man nor the circumstances but the man-in-the-circumstances" that produces a great statesman. It appears that Khadduri would not count any of the twelve as truly great, although some approached that distinction.

Among the enduring monuments of the great Arab culture of medieval times are a number

of anthologies that utilize a unique form of biography to personalize and enliven discussion and understanding of abstract philosophical issues. Professor Khadduri's work revives that ancient and noble tradition of scholarship.

ROBERT G. LANDEN
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Arlington

AFRICA

K. INGHAM, editor. *Foreign Relations of African States*. (Proceedings of the Twentyfifth Symposium of the Colston Research Society held in the University of Bristol, April 4th to 7th, 1973. Colston Papers, volume 25.) London: Butterworth; distrib. by Shoe String Press, Hamden, Conn. 1974. Pp. xii, 344. \$32.50.

Since 1948 the Bristol-based Colston Research Society has sponsored symposia on a variety of subjects, ranging from cosmic radiation, hypersonic flow, and the fungus spore, to animal health and production. In 1973 the twenty-fifth annual Colston symposium was devoted to the historical origins and present-day manifestations of African foreign policy and relations. The essays that appear in this book represent the proceedings of that symposium and will be of interest not only to the historian and political scientist, but also to those concerned with contemporary African affairs.

The first six papers in this collection attempt to explore the relations between precolonial African societies, a relatively neglected aspect of African history. Topics include diplomacy and diplomats in the Western Sudan (Thomas Hodgkin), Fante diplomacy in the eighteenth century (A. A. Boahen), and the external relations of the East African coast (G. S. P. Freeman-Grenville). The remaining essays are more contemporary in nature, dealing with such topics as the colonial partition of Africa by treaty (C. H. Alexandrowicz), modern Tanzanian foreign policy (T. M. Shaw), the formations of foreign service cadres in black Africa (A. H. M. Kirk-Greene), and the Organization of African Unity (Z. Červenka).

One of the most interesting essays in this Colston collection is Y. Tandon's "Analysis of the Foreign Policy of African States: A Case Study of Uganda." Tandon advances the thesis that postcolonial internal development within many African states, Uganda in particular, has in large part been a reflection of former colonial relationships. Concurrently, it is maintained that the most important determinant

of modern African foreign policy, at least in the case of Uganda, has been the continued dependent relationship upon former colonial powers. In Uganda, for example, recent "internal" developments, such as the expulsion of the Asian bourgeoisie and the British capitalists, were merely a response to the colonial legacy "that had created *foreign* ownership *within* Uganda, and could thus be legitimately regarded as part of what was conventionally described as 'foreign' policy," as opposed to a purely "internal" phenomenon.

ROBERT R. DAVIS, JR.
Ohio Northern University

CHARLES-ANDRÉ JULIEN. *L'Afrique du Nord en marche: Nationalismes musulmans et souveraineté française*. 3d rev. ed.; Paris: René Juliard. 1972. Pp. 439.

IAN CLEGG. *Workers' Self-Management in Algeria*. New York: Monthly Review Press. 1971. Pp. 249. Cloth \$8.95, paper \$2.95.

A former professor of history at the Sorbonne, Charles-André Julien is the *doyen* of French North Africanists. He is also a man who throughout a long and productive career effectively combined scholarship and political commitment. Having been briefly attracted to the Communist party in his youth by its anti-colonialism, he later served as a high official in the Popular Front regime and remained a lifelong critic of colonial rule.

L'Afrique du Nord en marche is a fine example of *engagé* scholarship. Published in 1953, when colon interests seemed to have blocked the advance of Maghreb nationalism, it was written in the hope of a meaningful reform of French policy. These reforms now seem very limited. Julien argues at the end for autonomy for Tunisia and Morocco within a reformed French union and for a more complete integration of Algeria into France: a *collège unique* and equal representation in the French National Assembly.

The book is rooted in a profound knowledge of Maghreb society, of French policy there, and of the nationalist leaders, many of whom Julien knew personally. Though frequently tart in its comments about both short-sighted colon interests and ineffectual pro-consuls, it avoids the excesses of polemical history and provides a solid survey of nationalist movements in North Africa. And yet it is very much a document of its times. When it was written, Tunisia and Morocco had not yet been conceded independence. The Algerian war had

not begun. Julien has added a good bibliographical chapter, but he has not rewritten the book. That it holds up well is a tribute to Julien's insight and scholarship, and yet a similar account written today would be very different. Julien is very much concerned with French policy and the positions of various parties, but, surprisingly for a socialist, he presents little class analysis and little on the economics of colonial domination. His focus is on the elites who formed the movements. Though over eighty, Julien has promised two more books that may fill this gap, the first on "La Fin des protectorats" and the second on "La Guerre d'Algérie."

Clegg's book picks up where Julien leaves off. Clegg is a Marxist interested in the possibility of worker management providing a check on the bureaucratic elitism that has deformed most socialist revolutions. Algeria provides an interesting case study because in the chaos of the transfer of power, workers on colon-owned estates and factories took over these establishments, largely to keep them running and to preserve their jobs. Though briefly given a major place in Algerian ideology, *autogestion* was in the long run betrayed as much by its friends as by its enemies. One faction believed in the necessity of an avant-garde to guide the spontaneous instincts of the workers, while another feared the deformation of the revolution in the interests of a labor aristocracy. In the long run, *autogestion* has been emasculated by a managerial elite more interested in economic rationality than in labor democracy.

The weakness of Clegg's book is that it describes the emasculation without really detailing the experience of *autogestion*. More than half of the book is concerned with background, either on the question of workers' councils, the heritage of the colonial economy, or the nature of the revolution. As with many Marxist writers, Clegg's strength and weakness are rooted in a concern with general questions without a full analysis of the concrete experience he is supposedly studying. He is rather clearheaded on the self-interested motives of the workers, and in the end he rejects the idea of worker management as an answer to socialist elitism.

Though written with great clarity and generally sound, I question some of Clegg's ideas. Thus, I doubt that the market in the traditional peasant economy was regulated by social rather than economic considerations. I think he is probably right that peasants were not fighting for modern socialism, but they were not necessarily fighting for a return to the past. I also

find his critique of Fanon's analysis of Algeria overstated. It does not account for two factors that vitally affected the nature of independent Algeria: first, that oil revenues made possible the rapid elaboration of a managerial elite, and second, that with independence power moved swiftly from those who fought to an army that had been immobilized during the war in Tunisia. This army first brought Ben Bella to power and then Boumedienne. But it is clear that Clegg is correct in pointing out that the socialism of Boumedienne's Algeria is dominated by a managerial elite akin to Stalin's new class and is very different from what Fanon would have predicted.

Clegg's book asks interesting questions but is not yet the history of *autogestion* that is well worth writing.

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JOHN DUNN and A. F. ROBERTSON. *Dependence and Opportunity: Political Change in Ahafo*. (African Studies Series, 9.) New York: Cambridge University Press. 1973. Pp. xi, 400. \$19.50.

When observed under the microscope of perceptive scientists, the humble and peripheral acquire a more appropriate grandeur and centrality. If, as in the case of the Ahafo, they cannot claim these qualities in the real, larger polity of which they are a small part, they nonetheless can become analytically significant. The less-important units, skillfully observed, can be vested with an importance and relevance usually reserved for the more obviously dominant units.

The authors, a political scientist and an anthropologist, and well suited to each other, here identify the appropriate status and research utility of a little-known political sub-unit of Ghana in what is at once a diachronic and a synchronic study. Their particular research problem is to explain relationships between a postcolonial state and its citizen-subjects in a national subregion it seeks to control. The time period under intensive scrutiny is 1968–71, with 1969 as a highly revealing year. The events of the period cannot be explained, however, without extensive reference to the peculiar history of the region over a period of about seventy years. Hence, this is a work the authors modestly introduce as a "studied confusion of academic genres, of history, anthropology, and political science." Actually, their modesty is belied by the assurance with which they reconstruct, record, and analyze the events

and processes of this complex area. Any confusion—using the term in its more popular sense—derives from the rather disordered, though not chaotic, and little-understood state of affairs in postcolonial Ahafo as it is perceived by the Ahafo themselves.

The remarkable breadth and detail of their material is presented in chapters on changing communal, geographic, ethnic, and political identities, economic development, colonial transformation, chiefship, politics of the sub-region, local government, and national party politics. From it emerges, above all, the certain sense that the Ahafo are uncertain about their relations with each other and about their position in the nation, and even more uncertain about their relationships to the dominant forces of the impersonal world markets on which their welfare has now come thoroughly to depend. Ahafo seem to represent many other plain but at once extraordinary people.

The techniques of research are those of intensive documentary study and field research. The authors gained unusual access to voluminous public records, interviewed extensively, primarily with open-ended schedules, and intelligently observed the public and many of the hidden aspects of political competition, conflict, and decision making. The detailed reporting blends information from the three disciplinary perspectives with confident surmises about the presumed meaning of events. These occasion some shadows of doubt, since many of the judgments can only be based either on the not always verifiable recollections and statements of less than objective or even knowledgeable or self-aware informants or on the logical deductions of the informed but outside observers themselves.

The prose style, furthermore, varies widely from simple sentences that are immediately and clearly comprehended, to complex sentences and ideas, deserving rereading, to elegant convolutions that require two or three readings when one should have sufficed.

This is a valuable book for scholars in the three disciplines, especially for those interested in social units larger than the local community but smaller than mankind, and for Africanists in general. It is, in addition, a pleasurable book for anyone who has ever lived in or heard of Ahafo.

ROBERT A. LYSTAD
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CHRISTOPHER FYFE. *Africanus Horton, 1835–1883: West African Scientist and Patriot*. New

York: Oxford University Press. 1972. Pp. xi, 169. Cloth \$6.75, paper \$1.95.

GORDON MACKAY HALIBURTON. *The Prophet Harris: A Study of an African Prophet and His Mass-Movement in the Ivory Coast and the Gold Coast, 1913-1915*. New York: Oxford University Press. 1973. Pp. xv, 155. \$7.50.

The history of the colonial era cannot ignore both the pathos and the accomplishment of those who sought to serve their people by serving the empire, only to be rebuffed and largely forgotten. It is not the least of the merits of Hollis Lynch's series of short biographies of black Americans and black Africans for Oxford University Press that he makes it possible for us to look closely at their lives in their times.

At one level, nothing could seem more dissimilar than the careers of James Africanus Beale Horton (1835-83), a West African scientist and patriot, as Christopher Fyfe calls him in the subtitle, and William Wade Harris (1865-1929), the African prophet who led a mass movement in the Ivory Coast and the Gold Coast in 1913-15, as Gordon MacKay Haliburton's subtitle tells us. One was a Sierra Leonean trained as a medical doctor in Britain, who served in the British army for twenty years, published extensively in medicine, botany, and geology, was a guiding spirit behind the proposals for self-government of the Fanti Confederation, and ended his life as a successful entrepreneur. The second was a man of much more limited education. After serving as a government interpreter and mission school teacher, he is said to have led a small revolt of Grebos against the Liberian government (the symbol of revolt being the hoisting of the Union Jack), went to prison, there saw the Archangel Gabriel in a trance, and emerged bearing the message of God to wander and to preach. This he did with whirlwind and long-lasting success in the Ivory Coast and the Gold Coast, converting many to his version of Christianity, only to be ultimately expelled. Harris ended his life in poverty and obscurity.

One need only look at their photos in the two books to observe the contrast. Africanus Horton stands erect in the stance of the British officer with sash, ceremonial sword, glorious hat, a sort of pompom in his right hand, and sporting the clipped imperial moustache. The Prophet Harris is in a long white robe and turban, black and full bearded, a cross and a long black tape as pendants. He is carrying in his left hand "a small Bible, a bowl for water, and a calabash, dried and covered with a

beaded net so that it clashed musically when shaken sharply" (p. 29), and a six-foot bamboo cross in his right hand.

And yet the similarities are fundamental. First of all, the message is the same: learn, adapt, and be of good cheer. For Africanus Horton this meant encouraging Africans to get the maximum amount of formal education, to assert and employ their talents as "modern men," to engage in practical scientific research on their own environment, to promote sanitary and medical reform, and to take initiatives in the political and economic "development" of West Africa. All these activities were suffused with an enormous optimism that leads Fyfe to say: "Horton could be unhesitatingly acclaimed as the prophet of the new Africa" (p. 158).

If one bears in mind that the recipients of the message of the Prophet Harris were less the urban educated to whom Horton spoke but rather the uneducated new cash crop farmers caught up in "a particular nexus of tensions" (p. xii) resulting from the economic demands of the colonial situation, the message is still learn, adapt, and be of good cheer. He demanded that the villagers burn their fetishes, cease segregating in special huts women who were menstruating (and thus keeping them from working in the bush as usual), send the children to school, practice cleanliness. But he would not condemn polygamy, and he discarded European clothing for African dress. As Haliburton says, his views were "akin to progressive missionary ideas" (p. 137). Africans responded to his gospel, and consequently "he changed [their] despondent situation into one of hope and purpose, where people applied themselves to solving their problems from a new set of premises" (p. 139).

Such constructive agents of modernity ought to have been cherished and honored by the colonial authorities. And yet they were not. Rather, the response was one of uncertainty, ambiguity, and finally rejection. In the case of Horton, though he was one of the first two Africans to obtain a medical commission in the British army, he was also the last (in the nineteenth century). When he sought in 1872 a post for which he was eminently qualified by knowledge and experience, head of the medical department in the Gold Coast, he was refused it for reasons, so the archives reveal, of pure racist prejudice. As for Harris, although in 1914 Governor Angoulvant of the Ivory Coast welcomed his return from the Gold Coast because he "preached obedience to the Administration's authority, forbade the use of alcohol and converted the *féticheurs* who had been, for

a long time, the causes of revolt against the French" (p. 81), soon thereafter, out of an obscure sense of discomfort, the authorities expelled him to Liberia. The only arguments ever put forth in written form applied to a white English Protestant missionary debarred from evangelizing among the Harris converts. The lieutenant governor "stressed the damage which preaching at large would do to the war effort by deflecting the minds of the inhabitants from their duty" (p. 118).

The origin of this ambivalence is found in the inherent contradictions of colonial administration as a political mode of assuring economic extraction. On the one hand, both to facilitate the process of overrule and to keep costs down, African intermediaries (formal ones like Africanus Horton or informal ones like the Prophet Harris) were invaluable and indispensable. On the other hand, to maintain a network of European lower stratum intermediaries, racism was an inevitable ideological corollary. Thus, as Fyfe notes of Horton, "In these European possessions whites ruled and non-whites obeyed. All the European empires in Africa were empires of race, where there was little space for an educated African like Horton. Those with professional qualifications were squeezed out of government service and humiliated socially" (p. 154). But of course this was even true of the Prophet Harris.

And the response by Horton? "To the last he retained his faith in British benevolence" (p. 155). One of Harris's last acts was to pass the baton to European Methodist missionaries. Therein lay the pathos beside the accomplishment.

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KANNAN K. NAIR. *Politics and Society in South Eastern Nigeria, 1841-1906: A Study of Power, Diplomacy and Commerce in Old Calabar*. (Cass Library of African Studies: General Studies, number 128.) Evanston: Northwestern University Press. 1972. Pp. xviii, 324. \$12.50.

This book is an example of the very good historical scholarship now coming out through graduate study at Nigerian universities. It shows specifically how important and valid it is to look again at the period often dismissed by those enthusiastically—and rightly—embracing African African history. The period is "colonial," or on its way to being so, but it is not less African thereby, as this study shows.

The African complexities emerge here be-

cause the author, aware of many dimensions, has done a careful and thoughtful piece of research, not because he is an African or a European (for he is neither). He divides his work into eight chapters, beginning with a general historical and institutional description. He then takes up social, political, and economic factors separately, first from 1841 to about 1860, then from the 1860s through 1906, ending with a conclusion.

Through it all he tries to trace the dynamics of African change, never presenting some static "traditional" base line against which Europeans induce adjustment, and at the same time to keep hold of his theme—that "it was the logic of the internal politics and structure of the society which in pre-colonial days determined the areas and depth of change that external factors were able to effect" (p. 271).

Dr. Nair assesses important aspects of contact between Africans and Europeans. He deals with religious syncretism, connections between economic and religious change, the use by Africans of Europeans as counters in their own political and economic rivalries, African abilities to separate the parts of missionary education they wanted from those they did not, to name a few. His conclusions are not startling, but African historians need them: the field is still too new for one to be blasé about a sound case study supporting earlier hypotheses. He also argues interestingly that in Calabar "the greatest resistance to colonialism was not so much political as economic" (p. 285).

This book is not for newcomers to Nigerian history; even its maps require prior knowledge. Like much basic research now being done on the African past, it is full of intricacy, necessary both to show that Africa is as complicated a place as anywhere else and to fill great factual gaps; it would, however, have gained from a less convoluted style. It is valuable for those with enough background to appreciate it, but it also contributes to making possible one day an accessible synthetic treatment that can be historically sound.

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RICHARD N. HENDERSON. *The King in Every Man: Evolutionary Trends in Onitsha Ibo Society and Culture*. New Haven: Yale University Press. 1972. Pp. xvi, 576. \$25.00.

ELIZABETH ISICHEI. *The Ibo People and the Europeans: The Genesis of a Relationship—to*

1906. New York: St. Martin's Press. 1973. Pp. 207. \$13.95.

Our knowledge of Ibo history and culture is constantly being expanded. The works of K. O. Dike, E. N. Njaka, J. C. Anene, I. F. Nzimiro, C. C. Ifemesia, F. I. Ekejuba, U. I. Ukwu, F. K. Ekechi, and Thurstan-Shaw; the forthcoming histories of the Ibo by S. N. Nwabara and A. E. Afigbo; and my forthcoming *Igbo Spirit of Enterprise* typify this interest in Ibo historical and cultural writing. *The King in Every Man* and *The Ibo People and the Europeans* have added to this recent flourishing historical inquiry into the Ibo. Both books attempt to reconstruct the history and culture of the Ibo and, in the case of Henderson, precolonial Onitsha in particular. One weakness of Ibo studies has been the lack of attention given to the densely populated areas of central Iboland, especially Owerri. One hopes that this will be corrected.

In method Henderson and Isichei have utilized official colonial and missionary documents and archival and recent ethnographical sources. More important, these authors appreciate the value of oral tradition in the reconstruction of Ibo culture and history. Henderson's approach concerns the interrelationship between Onitsha institutions, ideas, and behavior before the colonial period. He discusses status, power, and social behavior on conceptual and empirical levels. Students of Ibo studies interested in the anthropology of Onitsha in the nineteenth century will find Henderson's discussion of religious symbols, rituals, kinship system, authority structure, and competition in Onitsha very stimulating and enlightening. However, *The King in Every Man* does not adequately serve the Ibo social historian. The institutions and ideas discussed lack the historical evolution that would make them useful.

Isichei in *The Ibo People and the Europeans* succeeds in enriching Ibo historical writing, especially in her ability to apply the expanding frontier thesis to Ibo history. This thesis examines the reasons why the missionaries, the traders, and the English government penetrated the interior of Iboland and, more important, how these cultural, economic, and political forces finally settled in Iboland. Isichei is best when she discusses the slave trade. The brutalities of the slave trade are well presented. The ruthless economic plunder of Iboland is adequately documented. However, the pollution of Ibo religion by contact with Europe and the

disintegration of village solidarity are not well presented.

Some sections of *The Ibo People and the Europeans* reinforce stereotypes. For example, we are told that "some Ibo groups probably suffered more from Abam raids than they did from British military expeditions." We must, after all, bear in mind that such statements as this have no historical foundation. In fact, in her attempt to give us a moral balance sheet of Ibo-European contact, Isichei sometimes demonstrates an apologetic state of mind.

Finally, Isichei raises an important question that she cursorily answers. How does one explain Ibo response to the "new opportunities" of British imperialism? In explaining Ibo response to British presence in Iboland, we should perhaps look at Ibo cultural endowment, social organization, stress on alternative choices and goals, individual initiative, and recognition based on achievement. V. C. Uchendu, Simon Ottenberg, D. N. Levine, and I have considered these ideas to some extent.

Two points need to be brought out in the discussion of precolonial Ibo society as presented in the two books under review. First, the term "spheres of influence" is used but never clarified, especially in relation to Iboland. The questions that needed to be asked are: Did Onitsha, Nri, Arochukwu, Awka, and other Ibo city-states carve out spheres of influence in Iboland? If so, how was this achieved? Were these city-states successful? Second, Isichei and Henderson have accepted the popular notion that precolonial Iboland had "unusually open social systems" and "face to face democracies." Was precolonial Iboland really egalitarian? To me the question has not yet received a critically documented answer.

On the whole, *The King in Every Man* and *The Ibo People and the Europeans* have a decent respect for Ibo culture and tradition. They should be considered significant contributions to Ibo studies and worthy addition to any university library.

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E. E. EVANS-PRITCHARD. *The Azande: History and Political Institutions*. New York: Oxford University Press. 1971. Pp. xvi, 444. \$13.00.

Sir Evans-Pritchard, perhaps the foremost social anthropologist in Britain of his time, is best remembered by historians for his manifesto *Anthropology and History of 1950* (1961), which

systematically assailed the then antihistorical character of social anthropology and pleaded for a sociological history common to both disciplines. He had practiced what he preached since 1926–30, having been convinced by the Zande situation itself, or perhaps the Zande, that he needed historical depth to understand this society (pp. ix–x). He published more than a score of articles in various, sometimes inaccessible, journals after 1955. Seventeen of these are now brought together under one cover, making them available to a wider readership.

The organization of the four-part book is somewhat unusual. It begins with a census of clans, totems, and ethnic groups in 1930. Only about thirty-eight per cent of the population was of "true" Zande origin, while the *auro* or subjected peoples made up the rest, and these peoples belonged to many different cultural traditions. Such is the complexity of Zande society. This census presents the remaining evidence of conquest, a conquest that ended by 1905 but which had begun at the earliest in 1760. To describe the situation in 1930 was a presentation of ethnographic evidence in history.

The second part deals with the components of Zande culture as it was between 1905 and 1920. Evidence of linguistic borrowing and ethnographic data are used. Material culture and technology are stressed here, but some data about oracles, medicine, and religious movements are included. The evidence is once again presented with care and rigor, but a historical interpretation of the economic changes implied by the data is lacking. And this is even true for most of the religious changes implied, even though this was the facet of culture in which his anthropological work showed an unrivaled mastery!

The last chapter forms part of the two last parts, which deal with the political and military history of one kingdom (Gbudwe's) and describe its organization and the implied dynamics. The sources embody most of the traditions assembled by the author, which may be, as he claims (p. i), irreplaceable. Moreover, here one witnesses a dazzling display of ethnography. The master exhibits his virtuosity as a great ethnographer, but underneath the performance one is aware of a purpose. In the background the attentive reader detects the majestic interpretations that make sense of all the kaleidoscopic facets described.

In two ways this work is unique: first as a definitive record of oral data (p. v), then as a sociological history that uses one case to detail

the internal dynamics, the inner history of the Zande. Other historians are working on Zande history now and will be able to reconstruct external history better, certainly after 1850. The application of other techniques to the data assembled may yet deepen our understanding of the wider phenomenon: the rise of a set of kingdoms on the Ubangi/Uele, their spread and transformations from around 1700 onward. But as a model in its two unique aspects, this work constitutes the legacy of its author to African historians.

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STEVEN FEIERMAN. *The Shambaa Kingdom: A History*. [Madison:] University of Wisconsin Press. 1974. Pp. xii, 235. \$12.50.

Professor Feierman's admirably lucid study is among the finest histories of an East African people yet produced by the new African historiography. Indeed, it is considerably more than that. Based on two years' field research collecting oral traditions, as well as the sifting of more familiar historical sources, *The Shambaa Kingdom* is the first of two integrated studies, the second of which examines Shambaa culture and society. According to Feierman, "All of my research on the history of the Shambaa has been an attempt to demonstrate that African history can be written for an audience which is western in orientation, in approach to historical issues and critical method, without losing sight of the point of view of the actors themselves" (p. 3). This is a demanding task and his success throughout is striking.

Method, both historical and anthropological, is also at the heart of this book. Feierman's mastery is most impressively displayed in his stimulating analysis of the myth of Mbegha, the eighteenth-century founder of the Shambaa kingdom. His diligent field research and his scrupulously careful handling of oral sources enable him to arrive at an understanding of both the myth and the history of Mbegha that few others have achieved in similar cases. His analysis is full of suggestions for historians and anthropologists alike. Impressive, too, is the author's initial chapter on Shambaa society, which displays an acute appreciation of ecological factors in the definition of ethnic identity.

No less commendable is his penetrating discussion of Shambaa political economy during the nineteenth century. By recognizing the motor force of economic change driven by international trade during this period, which he

points out is not the view of his informants, Feierman demonstrates the overriding importance of reconstructing a microhistory such as this in its wider regional and international context.

My only complaint about this excellent book concerns Feierman's frequently tantalizing references to fuller discussions of aspects of Shambaa society in his unpublished companion study. But when we have this volume, too, we can anticipate as arresting a reconstruction of the history and culture of a precolonial African society as we now possess.

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ALLEN F. ISAACMAN. *Mozambique: The Africanization of a European Institution, the Zambezi Prazos, 1750-1902*. Madison: University of Wisconsin Press. 1972. Pp. xviii, 260. \$17.50.

The owners of *prazos* in Mozambique have long attracted comment for their unique blend of European and African life-styles over nearly five hundred years. A careful study of the heyday of this institution, such as Allen F. Isaacman has here presented, is thus most welcome.

Documentary material in Portuguese, Mozambican, and British archives has been tested, weighed, and balanced with extensive interviews and the collection of scientifically controlled oral historical data in five sections of the Zambezi basin. The study focuses upon the last 150 years of the *prazo* system but is by no means limited to that period in its implications or contribution. It is not a little surprising, and much to the author's credit, that he has so much success working in the field during the days of the Salazar-Caetano domination of Mozambique.

The result is a careful, but readable enough, narrative of the arrival and intermingling of Europeans with resident and migrant African societies in southeastern Africa. In a sense the *prazo* became a quasi-feudal landholding—certainly it was a title and a power base recognized, in some fashion, by Portuguese law—but it also became a part of the fabric of African life and was strongly influenced in many of its forms by African customary and political traditions. Indeed, most of the *prazeros*—especially the successful ones—played a delicate game of balancing marriage policies between European lineages and African alliances, and they maneuvered, like fiefdoms, between alliances and opposition (including direct, violent action)

among one another and between themselves and their African neighbors. Thus, Africans were sometimes wards, sometimes allies; at times Africans held an upper hand in this balancing, perhaps as often as the relatively weak and distant colonial government. But the *prazero* benefited from the power to balance these factors at the fulcrum of practical, day-to-day regional politics and society.

Isaacman has given us details by *prazero* families, by individual African societies, and by the judicious use of all types of sources illuminating the subject. The result is a significant contribution that will be one of the models of historical analysis in societies and situations of this type.

The bibliography and notes are ample, the index is good, and the maps are quite satisfactory supplements to the text.

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ASIA AND THE EAST

T'UNG-TSU CH'Ü. *Han Social Structure*. Edited by JACK L. DULL. (Institute for Comparative and Foreign Area Studies, University of Washington. Han Dynasty China, volume 1.) Seattle: University of Washington Press. 1972. Pp. xix, 550. \$17.50.

In his foreword to *Han Social Structure* Karl L. Wittfogel writes, "While here and elsewhere different interpretations can be expected, all serious students will agree that Professor Ch'ü's contribution has raised the investigation of Chinese society and history to a new level of factual inquiry and coordination." This is a very fair statement. The tremendous amount of source materials used and made available to students with or without a good knowledge of the Chinese language sets a sound landmark. It is also Ch'ü's contribution that he blends legal history into this study.

The book is divided into two parts, "Analysis of Han Social Structure" and "Documents of Han Social Structure." The former contains five chapters; the latter, three sections; each occupies approximately 250 pages. The bibliography and index will prove very useful.

With a preconceived topical analysis of Han social structure, Ch'ü inadequately treats social change and development, even though he was free to draw materials from the dynastic histories within a span of four hundred years. More space was given to the upper (pp. 66-101, 160-245) and lower (pp. 135-59) strata of Han

society than to the "commoners" (pp. 101-27) —the scholars, farmers, artisans, and merchants, as carriers of education, producers of food and commodities, manufacturers of salt, iron, fuel, paper, and tools, and distributors or traders, domestic and "international."

Following the German economic historian and sociologist Max Weber (1864-1920), the author analyzes social structure through the possession of economic goods, political power, and social prestige in the ruling bureaucracy. Weber stressed the relationship between religious ideas and economic development; but such was not true of China. Chinese civilization has been humanistic; of the twenty-six dynastic histories, sixty-two per cent are biographies, twenty-one per cent treatises, eleven per cent annals of emperors, and two per cent annals of nobles. Perhaps this accounts for extreme brevity of the treatment of religious function in Han society. Ch'ü draws more heavily from biographies than from any other section.

Treatment of the position of women (pp. 49-62), guests or retainers (pp. 127-35, 444, 454), *Yu-hsia* or wandering knights (pp. 185-95, 245-47), and *Jen-hsia* or redressers-of-wrongs (pp. 186, 393, 413, 420-24, 440, 449) illumines Chinese society. With a deep interest in legal history, Ch'ü gives a good résumé of opinions from leaders of all schools of thought (Confucian, Moist, Legalist) regarding *Yu-hsia* and *Jen-hsia* as violators of law in making illegitimate use of coercive power and violence (p. 190). Due to their valor and unselfishness, *Yu-hsia* and *Jen-hsia* were admired, and they gained many followers throughout Chinese history in one form or another.

Even though monographs on administrative geography, agriculture, communications and transportation, temples and monasteries, and the western regions are in progress, this volume needs an introductory chapter with a graphic sketch of these aspects whereby the reader can see the setting for the Han social structure. Further, the dates given on page margins in part 2 seem arbitrary, with two hundred years for each section, one hundred for B.C. and one hundred for A.D. No doubt, it serves some purpose; then, why not apply this chronology to part 1?

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THOMAS A. METZGER. *The Internal Organization of Ch'ing Bureaucracy: Legal, Normative, and*

Communication Aspects. (Harvard Studies in East Asian Law, 7.) Cambridge, Mass.: Harvard University Press. 1973. Pp. x, 469. \$20.00.

In this architectonic work, Metzger proclaims further revisions in Confucian studies. Marx overstressed the economics of the ruling class; Weber, with preferable insights, erred in positing the bureaucrat's comfortable ideological adjustment; Wittfogel overplayed old China's despotism; Eisenstadt is misled in supposing no "transformative quality" or pressure for change in the Ch'ing government. Another cliché: the late Ch'ing bureaucracy was corrupt as well as reactionary.

Metzger makes his amendments in a detailed study of Ch'ing *ch'u-fen*, or law of administrative punishments, with most of his rich case material drawn from the salt administration. The theoretical framework is clear throughout. We see one of government's antinomies: the need to foster effectiveness in the bureaus while somehow keeping and rationalizing the indispensable sanctions and restraints.

To the received tradition that Chinese scholar bureaucrats were amateurs despising legalisms, Metzger opposes a detailed demonstration of concern with the intricate regulations. Impeachment was a constant, but this led not to pervasive terror; rather, Metzger argues, it returns us to the ambivalence of canonical Confucianism. Human nature was not always called good; the world was assessed as "partly bad." Confucianists had a "charismatic sense" of their central role in saving their world, but, per contra, these same men were raddled by a sense of their failure in that mission. They expected punishment; sometimes they impeached themselves in calculation of it. Metzger tangentially links their "probationary ethic" with the Protestant ethic, but he stresses among the contextual differences the familiar fact that China's official offenders, even after stigmatization as criminals, could be reappointed or continued in office, without rank, in expiation. Indeed, the Ch'ing bureaucracy was more distinguished by "functional" than "dysfunctional" patterns; its "moderate realists" were more likely to strive and worry than to evade and line pockets. It really worked quite well.

Metzger has given us another of the newer institutional-legal studies designed to peel back the macroscopic generalizations that obscure "Confucian" reality. His tracing of Ch'ing administrative law is masterful, and his glossary, appendixes, bibliography, and index are solidly

interlinked. But if this structure must support the underscored proposition (pp. 412-13) that the "moral tension [of the bureaucrats] . . . was connected to the propensity of the Ch'ing elite to think in reformist terms, a propensity which certainly facilitated that transformation of Chinese society achieved in this century," there will be readers who will protest that the burden is too great.

Interpretations aside, Metzger has made a fundamental contribution to Ch'ing studies. Such meticulous calibrations must bring us ever closer to the reality.

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YEH-CHIEN WANG. *Land Taxation in Imperial China, 1750-1911*. (Harvard East Asian Series 73.) Cambridge, Mass.: Harvard University Press, 1973. Pp. xiii, 172. \$8.95.

YEH-CHIEN WANG. *An Estimate of the Land-Tax Collection in China, 1753 and 1908*. (Harvard East Asian Monographs, 52.) Cambridge, Mass.: East Asian Research Center, Harvard University; distrib. by Harvard University Press, Cambridge, Mass. 1973. Pp. xi, 57. \$4.50.

In the field of Chinese history, where political and cultural studies have traditionally dominated, more precise economic analyses have recently made significant contributions. Yeh-chien Wang's concise book on land taxation in late imperial China is an important and valuable study of this kind. It applies the exacting quantitative methods of contemporary economics to the traditional mode of institutional analysis. The result is a study that will stimulate both economic specialists and general students of China.

Wang's study is limited in scope but broad in its implications. Making extensive use of primary sources, especially the provincial financial reports compiled just before the revolution of 1911, he carefully and meticulously reconstructs the land tax collection system, its changing relationship to other sources of imperial revenue, the principles governing the tax rate and the methods used in tax collection, the variances from developed regions to developing regions in China, the impact of rising and declining prices on the real land tax burden, and precise estimates of the total income from land tax collection from each of the provinces. Detailed tables of tax collection figures supporting his general analysis are published in the companion monograph, *An Estimate of the Land-Tax Collection in China,*

1753 and 1908.

The broad implications of his findings challenge a widely accepted view that corrupt and oppressive taxation was an important factor in the fall of the Ch'ing dynasty. According to Wang, as the government required more revenue to pay for services to an increased population, war indemnities, and modernization programs, it established surtaxes and new taxes rather than increase the basic land tax. This collection of nonstatutory taxes had its own restraints and controls, worked "reasonably well," was widely accepted, and did not result in excessive exploitation of the peasant masses. He acknowledges that tax discrimination and deflation of prices created considerable tax burdens for the peasant before the Taiping Rebellion, but he does not see the same conditions existing before the 1911 revolution. In fact, Wang argues that in general the land tax burden and the overall tax burden on the Chinese citizen were less at the end of the dynasty than at its height. The land tax in Japan, he points out, was far more burdensome.

This view helps to explain the low level of overt peasant participation at the time of the 1911 revolution and corrects simplified oppression theories about the causes of revolution. Yet Wang's mathematical approach to determining the real tax burden of the peasantry is not wholly satisfactory. A sense of oppression also derives from social expectations, political loyalties, and other unquantifiable factors that cannot be put into formulas and statistics. In addition, it is not clear in his study how much of the nonstatutory taxes, aside from the land tax surcharge, might have been indirectly passed on to the peasant. In these matters there may still be room for debate, even though this fine study firmly demonstrates that the Ch'ing dynasty did not bring about its own downfall through excessive land taxation.

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JOHN R. WATT. *The District Magistrate in Late Imperial China*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press, 1972. Pp. x, 340. \$15.00.

The basic administrative unit in late imperial China was the *hsien* (district), which by the end of the eighteenth century often comprised some 200,000 people. Over each of the 1,500 *hsien* of the empire there presided a magistrate, responsible not only for the administration of

justice but also for the collection of revenue and the maintenance of order and correct ideology. What is remarkable is that the *hsien* magistrate was the only responsible imperial official in the huge area. Ever since the publication of T'ung-tsu Ch'ü's *Local Government in China under the Ch'ing* (1962), which generally confirms the findings of Kung-ch'üan Hsiao's equally excellent *Rural China: Imperial Control in the Nineteenth Century* (1960), it has been established that the formal government of late imperial China was extremely superficial and that the magistrate, despite his heavy responsibilities, was in fact quite impotent, having to rely on the informal government of the countryside—the village agents chosen by the “rapacious clerks and verminous government runners” that constituted the permanent subbureaucracy of the *hsien*; the local gentry; and the big lineages of south and east China. These facts are obviously of the greatest importance to any understanding of China's *ancien régime*, the background of her twentieth-century revolution.

Professor Watt's work confirms all this but raises vital historical questions not gone into by either Ch'ü or Hsiao. Part 1 includes newly discovered statistics that are generally compatible with the figures unearthed by previous scholars, indicating that the average tenure of the magistrates was short (say two to two and one-half years) and that among them about half were holders of higher degrees won through civil service examinations. Watt's data also show that only one-third of the magistrates had had previous administrative experience and that very few magistrates were promoted to higher offices, because of the high probability of their being demoted or dismissed under imperial “disciplinary regulations” particularly under the Ch'ing. Such findings, admittedly based on limited sources, support our present knowledge of the magistrate's helpless dependence on the despised yamen clerks and runners who could number in the thousands in a large *hsien*. The uncertainty of his career, as well as the need for financial security for himself and his family, gave the magistrate every incentive to acquire a competence in the few years that he was in office, while he did his best to deliver tax quotas and avoid blame for his handling of lawsuits. As social conflicts multiplied with the growth of population during the eighteenth century, many a magistrate whose Confucian studies had inspired him to work for the welfare of the people was becoming increasingly cynical regarding this ob-

jective. More often than not, he became a collaborator with local vested interests that, with the help of the clerks and runners, evaded taxes and manipulated justice at the expense of powerless peasants.

How then did China's local government ever come to this pass? It is to this historical question, never really raised by previous scholars, that Mr. Watt addresses himself. His findings, based on broad knowledge of China's cultural heritage as well as painstaking research and reflection, has earned for this book a place in the list of “must readings” on the history of imperial China. Watt has at least tentatively charted the field created by his own inquiry and, in the process, imparted insights invaluable to those who may pursue the subject further.

Watt dates the rise in influence of the *hsien* subbureaucracy somewhere between the fifteenth and sixteenth centuries, when the system of taxation and justice built up by the first Ming emperor, assigning responsibility to the local inhabitants themselves, was found no longer workable. With the collapse of the *li-chia* (neighborhood organization) and the *liang-chang* (principal tax collector) systems, there was no alternative but to entrust these functions to the yamen clerks and runners and the village agents they designated. Watt emphasizes the difficulties posed by the rise of powerful landlords during the sixteenth century, as well as the increased administrative burden on the magistrate resulting from the consolidation and monetization of rural levies under the “single whip” tax reform. These are questions that other students will undoubtedly pursue in greater detail, particularly since extensive literature by mainland Chinese and by Japanese historians awaits analysis. Despite Watt's pioneering work, the nagging question remains: why was it that after the early Ming period no effort was made by China's rulers until the twentieth century to extend formal bureaucracy deeper into the rural society?

As Watt has emphasized, everything that the imperial rulers did during the early Ch'ing period merely made the corrupt subbureaucracy more indispensable. While the tenure of magistrates was once nine years during the Ming, it was greatly shortened under the Ch'ing. At the same time, the magistrates' salaries became utterly inadequate, in part owing to rising prices. Mr. Watt invokes the ancient Legalist concept of the “public” and the “private” (*kung* and *ssu*) in explaining Ch'ing policy. He believes that the early Ch'ing emperors, while

espousing Confucianism, often practiced Legalist methods. It was the imperial strategem to maintain that all tax revenues were for "public" purposes, not for the personal benefit of the bureaucrat. " 'Private' became synonymous with 'illegal' and could no longer coexist with 'public' " (p. 162). One can only regard such an imperial posture as hypocrisy on a gigantic scale. The early Ch'ing monarchs were magnanimous enough to set very low permanent rates for the land tax. Yet the merely nominal salaries of the officials made the proliferation of surcharges and "customary fees" unavoidable, and the burden was bound to fall on the weak and the poor among the taxpayers. Mr. Watt has brought into sharp focus the predicament of conscientious Confucian magistrates. While they were ethically obligated to tend to the needs of the populace, they were, under the system, "losing the means and the will to be close to the people" (p. 233). It may, however, be suggested that the Ch'ing emperors were not alone to blame for this double standard. Among the advisers of the K'ang-hsi and Yung Ch'eng emperors were many earnest Confucians. During the latter reign, it was the counsel of numerous provincial and metropolitan officials that prompted the monarch to reject a proposal that there should be more regular imperial officials at the sub-*hsien* level to help restrain the yamen underlings (pp. 226-27). It is possible that Confucian concepts of "public" and "private" were themselves accommodating to a double standard. Socially approved aspiration for family fortune was often regarded as compatible with devotion to the public good. A scholar-official, sincerely trying to do his duty toward state and people, could quite without pang of conscience accept a share of the lucrative "customary fees" for private purposes. Mr. Watt has made a signal contribution by inquiring into the origins and nature of such moral and legal ambiguities. But further research is needed to determine whether the ambivalence was not in fact rooted in Confucianism itself and whether Chinese administrative institutions had, long before the Ming-Ch'ing period, been pointing this way.

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Davis

TRYGVE LÖTVEIT. *Chinese Communism 1931-1934: Experience in Civil Government*. (Scandinavian Institute of Asian Studies. Monograph Series, number 16.) Lund: Studentlitteratur. 1973. Pp. 290. 30 S. kr.

The Kiangsi soviet period was a formative one during the Chinese Communists' apprenticeship to full power, which they served in many areas of China before 1949. Among the principal reasons the Kiangsi soviet deserves close attention from students of modern China is that in its institutions were prefigured many later Chinese successes, failures, and problems. Trygve Lötveit's study, originally written as his thesis at the University of Leeds, contributes copious details of Chinese Communist administrative techniques and organization in Kiangsi. It therefore not only adds to our understanding of the early period, but it will help to demonstrate continuities in many practices.

At one level the author is interested in tracing the leftward movement in policies that followed Mao Tse-tung's temporary eclipse by the "returned Bolsheviks" in 1934. He finds this direction particularly noticeable in the punishment of criminals and the treatment of landlords and rich peasants, and he conjectures that "radicalization" damaged the links between the masses and state and party apparatus that had been forged previously. But the chief value of the book lies in the author's close study of sparse sources of information on Chinese administration. Relying principally on the *shih-sou* collection of documents seized by the Chinese Nationalists in 1934, which is available on microfilm at a number of major libraries, Lötveit describes the structure of a variety of Kiangsi governmental institutions. He first discusses administration at every level from the *hsiang* upward to the central government. His particularly detailed chapter on the administration of justice is the longest study in English to appear anywhere on that subject and contains helpfully substantial excerpts from policy discussions and reports of decisions. He is also thorough on land reform policy and finance.

The book is essentially descriptive and its generalizations are few, yet the material presented affords glimpses of early Chinese Communist experimentation in government and of the problems that new techniques encountered. These insights are apposite to the study of China today. A prominent tension since 1949 in the People's Republic of China has been between mass mobilizational techniques and more bureaucratized means of implementing policies. In Kiangsi, Lötveit shows us, the same tensions existed. For example, there was considerable conflict among Communist leaders over the extent to which decisions on criminal cases should be influenced by expressions of mass sentiment, as opposed to judicially or

administratively determined standards. In the land investigation campaign of 1933–34, the terrorism of landlords and rich peasants was followed first by an easing of tensions, then by renewed harshness toward those targets. That campaign created problems of a type that latter-day Chinese leaders encounter in using mass campaigns as administrative devices. Changes in the direction and interpretation of policy and the resulting uncertainties cause confusion and endanger cadre careers. Mao himself was severely criticized after the land investigation campaign. The persistence throughout the history of Chinese Communist governance of some of the difficulties that Mao and his colleagues encountered in Kiangsi over forty years ago will help to maintain the usefulness of this book.

STANLEY LUBMAN
Washington, D.C.

ILPYONG J. KIM. *The Politics of Chinese Communism: Kiangsi under the Soviets*. (Sponsored by the Center for Chinese Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1973. Pp. 232. \$12.50.

One of the least-known and researched periods in modern Chinese history is the so-called Kiangsi soviet period, that is, the interlude commencing with the establishment of the Chinese Soviet Republic at Juikin, Kiangsi province, in November 1931 and ending with the pull-out of the Red Army and Communist adherents for the Long March in 1934. The contemporary accounts in Western languages are sparse, and even Mao's officially published works (*The Selected Works of Mao Tse-tung* [1960–]) contain only four of his addresses and reports of these vital and formative years in the evolution of the Chinese Communist party and its state. Thus this long-awaited study, commenced more than a decade ago by Ilpyong J. Kim, is a welcome addition to the literature of the field. How did Mao and his colleagues make the necessary transition from a patch-quilt movement of dissatisfied intellectuals (some Moscow-trained), urban and rural dissidents, and scattered guerrilla forces to the purposeful, seemingly relentless revolutionary force that took over the whole of China by might and popular will in 1949? Numerous accounts picture the Long March as a modern-day "forty years in the wilderness" during which stragglers and dissidents were shorn from the main, pure body and the party and its army

were honed into an efficient sociopolitical and military mechanism. Following the lead of the official Peking version, most recent accounts stress the importance of the immediate post-Long March period (1935–48) during which the Communists established their (semi-) autonomous base areas in Northwest China, what Mark Selden has so aptly termed the "Yenan Legacy." For by the time they retreated from Yen-an in order to take Peking and the whole of the mainland of China, Mao not only had established his primacy in the party, but he had made his techniques of mass rural organization and policy implementation a successful reality for hundreds of millions of oppressed Chinese. This stress on the purification rite of the Long March and creativity of the Yen-an period would seem, on the basis of Kim's study, to have blotted out the achievements of the preceding Kiangsi soviet and created the impression of groping and leaping, of suddenly gained intuition, where in fact there had been steady evolution and development. In short, he demonstrates that the Yen-an regime, and indeed the major policies of the People's Republic under Mao, are outgrowths, refinements, and elaborations of the wedding of practice and theory that Mao was already achieving in Juikin, Kiangsi. On the basis of solid data and careful analysis, Kim opens more fully than ever before the world of the Kiangsi soviets, providing details of the structure, leadership, administration, behavior, and policies of the early Communist period and the first Communist government in China. He singles out the three basic components for the seizure of political power, as perceived by the party leaders, as "agrarian revolution," "armed struggle," and the building up of "base areas" (the so-called soviet republics). The soviets are defined here as techniques for mass participation and mobilization, and Kim correctly points out that they were in many cases far from spontaneous expressions of popular response. Rather Mao and his comrades had to find ways and means to rouse peasants from their lethargy—a lethargy known all too well in Oriental societies of the past—seize upon moments of wild but helpless rage and resentment, and channel them into steps toward social and economic transformation. Professor Kim brings us a Mao who, aside from being a philosopher, military strategist, and theoretician, is an intensely practical politician with an intuitive understanding of how an organization operates and can be used. His birth, background, and bitter early revolutionary experience had taught him how to mo-

bilize and manipulate the peasant masses (the "mass line"), but the Kiangsi soviet period provided the kind of orientation that made him a master of the leadership elite as well. Following this thesis, the convulsions of the latter-day Cultural Revolutions, which to bewildered outsiders seem chaotic and directionless, would fall into place as parts of a long-practiced, skillfully orchestrated political symphony, the music of the revolutionary spheres.

STANLEY SPECTOR
Washington University

HECTOR BOLITHO. *Treasures among Men: The Fudai Daimyo in Tokugawa Japan*. (Yale Historical Publications: Miscellany, 101.) New Haven: Yale University Press. 1974. Pp. xiii, 278. \$12.50.

The title of this absorbing book is from an anecdote in the *Mikawa Monogatari* extolling the virtue of perfect loyalty of vassal to liege. This pivotal value of the ruling class of Tokugawa Japan had special force in the vassalage of the *fudai daimyo*, the barons whose direct allegiance to the Tokugawa house following the Sekigawara victory in 1600 carried particular responsibilities in the national *Bakufu* government Tokugawa Ieyasu founded at Edo. Hector Bolitho disputes the convention that the *fudai* lords of domain adhered unswervingly until 1868, in act as well as in thought, to the strictures of selfless devotion to the sitting shogun. He contends that the calculus of personal advantage as landed magnates steadily ate away their special bond to the Tokugawa, and the initial distinction between *fudai* and *tozama*, the great independent *daimyo* defeated at Sekigahara who still presented powerful threats to Tokugawa pre-eminence, was increasingly blurred.

Bolitho sees the *fudai* as becoming more like *tozama* in their political and economic priorities, which reflected the regional concerns of the feudal estates more than those of their overlords in Edo. The *fudai*'s status as *Bakufu* officers made them eligible to the highest levels of *Bakufu* government but also imposed special obligation of fealty to the regime. Caught between responsibilities to their baronial estates, which growing *Bakufu* inertia made secure from attainder, and their hereditary retainer bond to the Tokugawa dynasts, their personal ambivalence fed political disequilibrium.

Reworking existing materials bearing on *fudai* brokering of incompatible tendencies, feudal anarchy versus monarchical absolutism,

Bolitho fashions a revisionist interpretation of the familiar theme of shaky balance between centripetal and centrifugal forces in the system. Instead of the standard view that after a brief power struggle the system froze at a slight cant that favored the *Bakufu* center for two centuries, that the regime's firm hold on this most reliable of feudal elements broke suddenly in the face of foreign intervention in the mid-nineteenth century, he argues that *Bakufu* power reached its zenith in the mid-seventeenth century. Its slow decline thereafter, prompted imperceptibly by the self-interests of the *fudai*, has gone largely unperceived. Historians have been taken in by the rhetoric of fealty that cloaked the *fudai*'s deeper concerns for technological and economic changes affecting their fiefs.

The introduction lays out the argument persuasively. The epilogue deals with the final rush of events culminating in *Bakufu* collapse, the naked revelation of progressive decay at the center. The four substantive chapters in between show eloquently how the peculiar split-level *fudai* relationship to Tokugawa feudalism, as regime officials and as lords of domain, and the assumption that fierce battlefield loyalties and landed estates are equally as transmittable lineally were fatal weaknesses of the Tokugawa system. While some of his supporting inferences require further substantiation, Bolitho's thesis as a whole is convincing. His illumination of the *fudai*'s key role in Edo politics sets a fine example for other rethinkers of Tokugawa history.

JOHN B. CORNELL
University of Texas,
Austin

WILLIAM B. HAUSER. *Economic Institutional Change in Tokugawa Japan: Osaka and the Kinai Cotton Trade*. New York: Cambridge University Press. 1974. Pp. xiv, 239. \$18.50.

This book gives us a new perspective on the economic role and significance of Osaka in the latter half of the Tokugawa period. Hauser shows that new patterns of trade and rival commercial interests had developed, making inappropriate the old description of Osaka as the "kitchen of Japan." Osaka continued to be the major center of economic activity, but her merchants were no longer capable of cornering the market, fixing prices, and browbeating even the city of Edo. Rural merchants now provided alternative outlets for village products.

Most of the above interpretations are based on the author's reading of recent works by Miyamoto Mataji, Yasuoka Shigeaki, Nakai Nobuhiko, Wakita Osamu, Hayashi Reiko, and others. He then turns to specific investigation of the cotton industry as it developed and affected the *tonya* and *nakama* of Osaka and the cultivators and rural merchants of the Kinai region. For these studies he relied on primary documents contained in the *Osaka Shishi* and the several local records of towns and cities in the Kinai region. By relying on secondary sources for the general framework and concentrating his attention on Osaka and the neighboring districts, Hauser is able to describe the nature of competing rural-urban interests and the *Bakufu's* role as arbitrator.

Hauser's main concern is not the cotton industry per se; rather, it provides the focus by which to examine the interaction between the Osaka monopolies and the cultivators and rural merchants who struggled against the restraint of trade and the domination of their economic lives by the *tonya* and *nakama*. In the dispute of 1823 involving over one thousand villages, the *Bakufu* compromised, preserving the rights of *tonya* and *nakama* within Osaka but permitting other marketing arrangements on the outside. This was a major victory for the rural interests, and it demonstrated the pragmatic attitude of the *Bakufu* toward the powerful economic forces of Osaka.

Hauser makes no effort to generalize from his particular conclusions, for he is aware of regional variations. He barely touches on the growing intrusion of *han* plans into this commercial situation. But he has helped to solidify our understanding of another unit of Tokugawa economic history.

ROBERT K. SAKAI
University of Hawaii

KOZO YAMAMURA. *A Study of Samurai Income and Entrepreneurship: Quantitative Analyses of Economic and Social Aspects of the Samurai in Tokugawa and Meiji Japan*. (Harvard East Asian Series 76.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xii, 243. \$11.00.

Professor Yamamura comes to economic history from economics in its quantitative tradition, but he takes no one-sided cliometric methodological stand. He is known widely for his study, *Economic Policy in Postwar Japan* (1967). With the present volume he turns his historical, econometric, and computerological expertise back in time to Yedo and early Meiji. Other

projects in progress under his direction will carry these skills yet further into the Japanese past, where he believes economic data are better, more voluminous, and less-thoroughly explored than for medieval Europe.

Two separate monographic studies are combined between the covers of this short book. Each of them challenges an accepted position. Part 1 (Tokugawa) diagnoses the "increasing misery" of the *hatamoto* or bannermen (non-coms and junior officers of the Tokugawa forces) as involving only disappointed "rising expectations." Part 2 (Meiji) denies, first, that Meiji entrepreneurs were primarily of samurai origin and likewise, second, that the samurai among them set their patriotism or their Bushido ethics (of primary Confucian origin) above standard microeconomic monopolistic profiteering.

Japanese economic historians are not only predominantly Marxist but, in Yamamura's view, are interpreters of Marxian "increasing misery" in an absolute sense. *Hatamoto* records of 1600-1800, plus price statistics from other sources, convince Yamamura that, first, *hatamoto* real incomes failed to keep pace with economic recovery from *sengoku* civil warfare but did not fall absolutely; second, *hatamoto* upward mobility (from lower to higher heirarchical ranks) tended to fall over these two centuries; and, third, *hatamoto* responses to their falling relative economic status included reduction in marriage rates and family sizes. (There seems little here to which Karl Marx's ghost would object.) Yamamura also finds it strange that *hatamoto* did not somehow organize to improve their positions; it seems to me significant, however, that the individual *hatamoto* and *gokenin* samurai participated so prominently in the anti-*Bakufu* movements culminating in the Meiji Restoration.

Yamamura's second group of theses relate to entrepreneurs in early Meiji. Contrary to conventional wisdom, he finds a disproportionate percentage of them to have been townsmen, with many of the ostensible samurai hailing from a marginal "beerage" or kulak subclass called *gōshi*. The main issue, however, seems less samurai participation in top-level entrepreneurship than the unusual absence of samurai opposition to the rise of Japanese "economic men." Yamamura's second claim, that even the samurai entrepreneurs acted like robber barons and/or profit-maximizers, seems based largely on a sample of two muckraking biographical sketches—of Iwasaki Yataro (Mitsubishi zaibatsu) and Yasuda Zenjiro (Yasuda

zaibatsu). It will be interesting to await the reaction of three "orthodox" authorities particularly, limiting ourselves to writers in English: Father Johannes Hirschmeier (*Origin of Entrepreneurs in Meiji Japan* [1964]), Professor Byron Marshall (*Capitalism and Nationalism in Prewar Japan* [1967]), and Professor T. C. Smith (*Political Change and Industrial Development in Japan* [1955]).

We find here not only the full bibliography and index hoped for in studies of this kind, but extraordinarily detailed statistical and quasi-statistical tables that other scholars, of all degrees of linguistic qualification, can use to review Yamamura's conclusions or extend his analyses in other directions. Both Professor Yamamura and the Harvard University Press deserve congratulations for their decision to include these materials in full.

M. BRONFENBRENNER
Duke University

HUGH TINKER. *A New System of Slavery: The Export of Indian Labour Overseas, 1830-1920*. New York: Oxford University Press, for the Institute of Race Relations. 1974. Pp. xvi, 432. \$18.50.

In recent years much more has been written about the international migrations of Chinese laborers than about the similarly massive movement of Indian coolie labor throughout the tropical world. This book provides us with the single most important work on this latter highly ramified subject. Its magnitude is, first, geographical: the migrations stretched from several regions of India to all the tropical portions of the British Empire, continental Africa, and Southeast Asia, as well as the islands of the Caribbean, South Pacific, and Indian Ocean. Further, the migrations were rooted in an international structure of trade and finance that changed by stages from the eighteenth-century slave system to the later system of indentured coolie labor. Several plantation crops came to be cultivated largely by Indian laborers; sugar was central, but tea, coffee, and other crops also bore both ecological and political significance. Finally, the markets for plantation products expanded rapidly throughout the nineteenth and early twentieth centuries in the urbanizing, industrializing populations of the white world. These worldwide patterns influenced the lives of millions of Indian workers in ways over which they had little control. The coolie system's machinery of oppression was supported by the empire's elaborate machinery of social

control, both legal—laws, courts, and police—and informal—planters and taskmasters.

Few historians would be adequately qualified to explore a subject of this magnitude; Hugh Tinker is one of them. Emeritus professor of government and politics at the University of London, he has had a distinguished career as an analyst of the Empire and Commonwealth. He handles the intricacies of imperial and parliamentary politics and ease and clarity. He does full justice to moral and attitudinal trends as well, as when he considers the antislavery campaign of the early nineteenth century and the post-1900 protests of the Indian National Congress against the conditions of labor in Africa and Fiji. However, he also sees how slowly and organically circumstances usually changed. Crucial to his perspective, as his title pointedly indicates, is the view that the change from slave to coolie labor was only a matter of degree before 1900. His account reminds us that there are many gradations of social servitude and that from the workers' viewpoint it is sometimes meaningless to distinguish slaveholding from nonslaveholding economies.

The most distinctive chapters in this ambitious work are probably the first three. Tinker's synoptic accounts of the transition from the slave-plantation economy to coolie recruitment, the tropical products of the nineteenth century, and "The People," give a vivid and precise description of the varied social and economic pressures within India's landscape that led workers to join the exodus. His many years' experience with India stands him well in delineating the movement of tribals, untouchables, farmers, Biharis, Tamils, and many others. These chapters set the framework for a volume that is complex, erudite, lucid, and passionate.

RICHARD P. TUCKER
Oakland University

TOM G. KESSINGER. *Vilyatpur 1848-1968: Social and Economic Change in a North Indian Village*. (Sponsored by the Center for South and Southeast Asia Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1974. Pp. xiii, 227. \$12.00.

A signal contribution of North American scholarship to Indian historiography in the last decade has been the growing focus on regional studies. The use of the cultural region (roughly analogous to the European national unit) as the

level of analysis has been very productive of new insight into the Indian past and frequently has shown the weakness of continental-scale generalizations previously advanced without an adequate foundation in specialized monographs. Tom Kessinger's splendid book may advance this process one step further. His intensive study of one Punjab village over the past 120 years refutes some accepted notions and challenges others.

Kessinger married into the Vilyatpur landowning caste—which one suspects gave him unique advantages in access to family and pilgrimage records. His detailed reconstruction of the fortunes of Vilyatpur's families lends convincing weight to his argument that models of "the traditional village" in the historical literature are useless because of their generality and timelessness. Traditional ideas and institutions, he argues, are themselves the products of long-term historical change and do not transcend time. Kessinger shows how these ideas and institutions have changed in Vilyatpur, sometimes in ways that involved major change for the villagers but nevertheless escaped detection by such normal outside sources as the census. Furthermore, Kessinger argues that without the historian's emphasis on change over time, village studies often miss the kind of transformation he is able to document for Vilyatpur, because the Western observer cannot believe that the "poor and primitive" village he sees before him is in fact the product of very considerable improvement in recent times.

What changes has Kessinger found in Vilyatpur? After a brief sketch of the village's origin in the Mughal period and of the political history of the area (the modern Jullundur district, Punjab) to the British conquest, he uses the detailed records available after that event for a reconstruction of the village in 1848, which is followed by an analysis of demographic change, of the agricultural economy, of other economic activity, and of village society (particularly family and kinship organization) over the 1848–1968 time span.

Village population was relatively stable due to a low survival rate until 1920; since that date it has more than doubled over the 1848 base, and a substantial number of migrants have left for work elsewhere. A classic case of surplus labor forced off the land? No, Kessinger argues. Agricultural production has more than kept pace with population growth, absorbing more labor—and at a higher standard of living—on less land as prosperity, in such forms as brick houses and sheds for electric pumps, has

taken fifty acres out of production. This, too, is in a village that was already intensively cultivated in 1848, with thirty-one per cent of the land then double-cropped (as opposed to seventy-one per cent double-cropped in 1968).

The 1890–1910 period was a turning point during which the landless artisan and labor castes emancipated themselves from the domination of the landowning Jats and established their right to live in the village and work elsewhere. Since then, "untouchable" sharecroppers have become wage laborers and raised their standard of living as well as their self-esteem. Artisans now produce for city markets, commute to work elsewhere, or run their own businesses instead of serving the landlords. Slow, incremental changes have ended the dominance of the small village society as a self-contained referent unit and a political system completely dominated by the landowning Jats, but nearly the same mixture of castes continue to live together in the village, despite the loosening of old ties and the growth of new opportunities outside of agriculture. The most important external factors appear to have been the spread of transport facilities and the market economy after 1860; beginning in the 1890s, the possibility of migration overseas and to the new canal colonies in western Punjab; the high mortality due to epidemic diseases in 1890–1920; the introduction of higher yielding varieties of major crops in the 1920s, followed by improved irrigation and more double-cropping; and the new political situation after independence in 1947 with development activity producing such inputs as electrification in Vilyatpur. Education, curiously, has not been a major factor so far, and villagers seeking work outside have not gone into white-collar jobs.

Kessinger's *Vilyatpur* is a well-documented success story. Is this a typical Indian village? It is a prosperous village in a prosperous area; its landowning elite are the typical farmers of the Punjab, but they differ from other Indian landlord castes in their willingness to do their own work with their own hands. There are obvious dangers in attempting to generalize from this study to the all-India level, and perhaps even the "North India" of the subtitle may be straining after too broad a relevance. Kessinger's demonstration in detail, however, that the family-centered and family farm-centered economic strategy of Indian peasants does indeed work and work very well transcends the particular setting of this one Punjabi village and will be of interest to economic planners and social scientists as well as historians. This

is an outstanding piece of work, ably and clearly presented. I hope, for the sake of our students at least, that Kessinger will add a bibliography to the next edition.

FRITZ LEHMANN

University of British Columbia

LEONARD A. GORDON. *Bengal: The Nationalist Movement, 1876-1940*. New York: Columbia University Press. 1974. Pp. xii, 407. \$15.00.

Mr. Gordon's revised Harvard dissertation is divided into three parts, each dealing with a succeeding generation of Bengali nationalists generally associated with the Indian National Congress. Frequently they were at odds with the national leaders of the congress.

Gordon has chosen life histories of a few prominent Bengali leaders to trace the course of nationalism and subnationalism in Bengal. Unfortunately he does not explain the criteria for his choice—for instance, for the period 1876-1904, why R. C. Dutt and S. Ameer Ali (both called writers) and not Surrendra Banerjea (called a politician)? The case for Ameer Ali needs to be explained with some conviction because he did not consider himself a Bengali. It is regrettable that the portraits of early Bengali leaders tend to be rather commonplace: Gordon calls his subjects ideologues but makes no searching analysis of their ideology.

Narration of political events is Gordon's forte, and the middle part of the book in which he summarizes events and condenses a number of published monographs, biographies, and autobiographies is written in brilliant prose. The portion on Aurobindo Ghose is quite moving; the one on M. N. Roy, though much too brief, is very much alive. He treats C. R. Das with sympathy, calls him the last Bengali Hindu who could be trusted by a Muslim, and takes a swipe at Broomfield for his view of Das and for utilizing the imprecise theory of *bhadralok* in his political analysis. It is unfortunate that Gordon does not press his points or offer cogent rebuttals, and thereby he leaves Broomfield's judgments and theory intact rather by default. The last part, one half of the book, covering the period 1918-40, which ended with Subhas Bose's defection to the Axis, contains the paradoxical opinion that Bose was the most popular Bengali leader yet without sufficient public support. Das had mass support, but we must assume him to be a less popular leader.

My ambivalence about the book arises because it falls between two stools. It is more than

a routine survey of nationalism in Bengal, but it is also much less than a monograph. It is based overwhelmingly on English-language printed sources. Students of Bengali history should be greatly relieved to know that despite all the rhetoric about Bengali separate identity in language and culture, it is quite possible to do a dissertation on Bengali nationalism by utilizing English-language materials. Either Bengali sources do not exist or they are unimportant. Macaulay seems to have been proven right.

BRIJEN K. GUPTA

University of Rochester

R. A. STEIN. *Tibetan Civilization*. Translated by J. E. STAPLETON DRIVER. Stanford: Stanford University Press. 1972. Pp. 333. \$10.00.

GEORGE WOODCOCK. *Into Tibet: The Early British Explorers*. (Great Travellers.) New York: Barnes and Noble. 1971. Pp. 277. \$8.75.

The two books reviewed here complement each other and bring together a wealth of information for the general reader. They are also studies that specialists in the field will want to review. Neither work pretends to present valuable new discoveries; their contribution is rather to assemble in one place information that would be difficult to find elsewhere. Professor Stein of the Collège de France, the most eminent French Tibetan specialist, presents an excellent summary of salient aspects of the society and culture of Tibet. Dr. Woodcock, a long-time student of Tibetan culture and history and the founder of the Tibetan Refugee Aid Society of Canada, presents in his book a very readable account of the first British explorers to penetrate Tibet—George Bogle (1774-75), Samuel Turner (1783-84), and Thomas Manning (1811-12)—men who made pioneer journeys to develop contact with the "medieval" civilization of Tibet discussed by Stein in his work.

Tibet is one of those subjects on which more has been written and less said than about any other subject that readily comes to mind. Stein's book, now regarded as a classic, is made available to the English reader for the first time. A masterpiece of scholarship in the original, *La Civilization tibétaine* (1962), this present publication is a text revised by Stein in collaboration with the translator, J. E. S. Driver, himself a Tibetan specialist. One of its most notable contributions is the inclusion of Tibetan and Chinese sources on the subject while also drawing upon a wide knowledge of relevant Western studies.

The book is organized into five parts, the first being "Habitat and Inhabitants," which includes data on historical and cultural, as well as physical, geography. A few interesting pages are included on how Tibetans themselves have traditionally viewed Tibet. Part 2, "Historical Survey," is a fine summary of traditional Tibet with but a page or so on the twentieth century. Part 3, "Society," maintains a diachronic approach, giving a historical perspective to the developmental process of Tibetan civilization, which so many treat flatly as an ossified culture. Particularly fine is the treatment of the family, although not in the manner of the sociologist or the anthropologist. One sees the great clans or lineages, the structure and dynamics of power and property, and a unique life style. The largest section is part 4 on religion, not surprising in view of the fact that Tibet has a "sacred" rather than a "secular" society. In the previous section, religion is treated as a social institution; here, various aspects of the Lamaist cult and doctrine are authoritatively presented. Part 5 is an introductory survey or summary of the arts and letters of Tibet and is probably the weakest. The book is well indexed, has a fair number of attractive original illustrations by a native Tibetan artist, and includes a useful eleven-page appendix of precise transliterations of Tibetan names, which can be unsettling to the uninitiated reader. The bibliography is one of the best and quite up to date. Those familiar with the original addition feel that the coverage is rather overambitious, but the recognized authority H. V. Guenther gives it unreserved praise. The nonspecialist will appreciate its succinctness and clarity but may at times feel overwhelmed with details. Although it presents a problem to most scholars, one notable resource that could have added a significant element to the book would have been the use of Japanese sources, in view of the fact that the best work on Tibet, in certain respects, is now being done in Japan.

Britain has loomed so large in modern Tibetan history and the British have played such an important part in revealing this nation to the world that it is fitting to have George Woodcock's book recounting the first stage of the process. He does not pretend to discuss Tibet in the way in which Stein does, but rather he gives the British approach to it, the fascinating stories of the trials and tribulations of the first men to enter, and the impression of that land upon them. Actually, it is more than this for it also sketches the essential

background of British Indian policy on the Tibetan frontier from the middle of the 1700s until the Younghusband expedition of 1904. The journeys presented by Woodcock, among the greatest of all time, were actually published long ago. These and their diplomatic significance were analyzed by Schuyler Cammann in his *Trade through the Himalayas* (1951).

The British travelers discussed are the first men to transmit to us in our language an account of the Tibetan people. They are sober accounts of early travelers, quite devoid of the romantic view of Tibet of our day that projects a land of spiritual mysteries and magical fantasy as in the writings of such persons as Alexandra David-Neel. First is presented the journeys of the matter-of-fact merchant administrators of the East India Company, Bogle and Turner. To these Woodcock adds a third account of the eccentric Thomas Manning, a friend of Charles Lamb, and includes some original research on Manning's later life and his children by a Tibetan wife. Manning was the first Englishman to reach Lhasa (1811) and the last until the Younghusband expedition (1904). In an introduction, Woodcock includes an interesting summary of earlier non-British, Catholic missionary travelers in Tibet, who are little known except to specialists.

While the journeys and explorations discussed by Woodcock concern a great trading corporation seeking commercial relations in the eighteenth and nineteenth centuries, they were a prologue for the political developments so important in this century. The twentieth century is beyond the scope of the book, but Woodcock includes an epilogue and relates the earlier events to later developments. The men treated are refreshing for their lack of heavy-handedness and saber rattling so characteristic of the European approach in Asia after the Opium War. It yet remains for someone to summarize the activities and contributions of the generation of Sir Charles Bell, Sir Basil Gould, and Hugh Richardson. Woodcock's book is easy reading, but it is by no means the scholarly contribution of careful research and analysis represented by Stein's work.

PAUL HYER

Brigham Young University

KALA THAIRANI. *British Political Missions to Sind: A Narrative of Negotiations from 1799 to 1843 Leading up to the State's Annexation*. New Delhi: Orient Longman. 1973. Pp. 193. Rs. 25.

The British annexation of Sind in 1843 is now

considered to be a prime example of the grasping aspects of British imperialism. Annexation occurred despite the willingness of Sind amirs or princes to accede to frequent British requests for a variety of political, economic, and territorial concessions. Thairani argues that the British conquest of Sind was only the culminating point of a policy initiated in 1799 with the dispatch of Nathan Crow to Sind. Crow was the first of several agents charged primarily to collect political intelligence, especially on the Afghan shah and his potential Russian ally, and secondarily to explore commercial opportunities. British interests in Sind remained chiefly political despite some mistaken ideas on the navigability of the Indus River, and Thairani concludes that the British disaster in Afghanistan during the early 1840s led to the annexation of Sind as a countermove to reaffirm imperial power. The desire of Charles Napier for personal glory only speeded up the action.

Robert Huttenback has already plowed much of this historical plot in *British Relations with Sind, 1799-1843: An Anatomy of Imperialism* (1962), and Thairani does not differ significantly from him in approach or conclusions. Both works are solid studies of diplomatic negotiations and the personalities involved. While Huttenback did research in British, Indian, and Lahori archives, Thairani has confined her efforts to Indian and Sindian archives. Her book contains much more detailed descriptions of British missions, their options, and their objectives than does Huttenback's. Even so her characterizations of people are one-dimensional, and she gives insufficient attention to the institutional framework in which they acted. The author delineates broadly the varying levels within the British imperial hierarchy, but she fails to mention such relationships as the brotherly one between Alexander and James Burnes. On the Sindian side there is even less information on the political arrangement by which the Talpur family controlled Sind. The founder, Mir Fateh Ali Khan, had ruled jointly with his three younger brothers as the *Char Yar* or four friends while his son established a branch in Mirpur and his nephew one in Khairpur. This complex political system seemed to invite divide-and-rule tactics, but nowhere is it related to Baluchi tribal or family structure, religious orientation (Sunni or Shi'a), or any other factors. There is, furthermore, scant explanation of why Khairpur was able to maintain a semiautonomous existence until 1947. One wishes that Thairani had moved beyond

Huttenback's study in scope as well as in depth of narrative.

BARBARA RAMUSACK
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G. F. MURASHEVA. *V'etnamo-kitaiskie otnosheniia XVII-XIX vv.* [Vietnamese-Chinese Relations, 17th-19th Centuries]. (Akademiia Nauk SSSR, Institut Vostokovedeniia.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 157.

If the present work is any indication, the time may soon arrive when American students in East Asian history will find Russian yet another obligatory addition to an already formidable list of basic language requirements. It is certain, at any rate, that if Murasheva's fine study of Sino-Vietnamese relations up to the time of the Opium War of 1840 had been written in some more accessible language, it would have been placed on our required reading lists by now.

Emphasizing the Vietnamese side of the Sino-Vietnamese relationship, the author directs her efforts to a re-examination of the disputed question of whether and to what extent Vietnam as a political entity was in fact independent of China despite the subordination or "vassaldom" that the traditional scheme of Sinocentric international relations demanded of it. She finds in the case of Vietnam the existence of a certain tension and the operation of a complex interplay between Vietnam's tributary obligations to China and its own quite independent interests that were not always in accord with Chinese preferences. After the tenth century, China's primary role in Vietnam was to act as the source of legitimation for Vietnam's ruling dynasties. China conferred this legitimation principally by granting the Vietnamese ruler investiture and accepting his tribute. These acts had, as the author points out, a very important impact upon Vietnam's political processes, and China's "interference" at this level was traditionally welcomed, even eagerly demanded, by the Vietnamese themselves. In other respects, however, Vietnam was quick to assert itself against its powerful suzerain. Chinese military intervention was successfully resisted on several occasions; border disputes were at times settled to China's disadvantage; and by the mid-nineteenth century, when Vietnam had achieved a high degree of political centralization after the Chinese model, it began to consider itself a better and more powerful example of Confucian civilization than its neighbor to the north, whose "suzerainty," however, it continued to recognize.

Murasheva is well read in Vietnamese historical sources and knows well the literature of the Fairbank school of traditional Chinese foreign relations. She does not, however, cite work in the Japanese language done in her field, and her use of Chinese sources is perhaps, as she acknowledges, less thorough than it might have been. Yet as she leads us through the details of Ch'ing China's policies and attitudes toward Vietnam, and Vietnam's toward Ch'ing China, her narrative is clear and lively and her general analysis perceptively cautious. Only the occasional choice of terminology (for example, "feudal leadership") indicates that this was a work written in the Soviet Union.

JOHN W. DARDESS
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CESAR ADIB MAJUL. *The Political and Constitutional Ideas of the Philippine Revolution*. With an introduction by LEOPOLDO Y. YABES. Rev. ed.; New York: Oriole Editions. 1974. Pp. xiii, 215. \$10.00.

OSCAR M. ALFONSO. *Theodore Roosevelt and the Philippines, 1897-1909*. Reprint; New York: Oriole Editions. 1974. Pp. xiv, 227. \$10.00.

PETER W. STANLEY. *A Nation in the Making: The Philippines and the United States, 1899-1921*. (Harvard Studies in American-East Asian Relations, 4.) Cambridge, Mass.: Harvard University Press. 1974. Pp. ix, 340. \$12.00.

The books reviewed here deal with American-Philippine interaction. Majul offers an analytical "history of ideas," developed notably by José Rizal, Emilio Jacinto, and Apolinario Mabini, that infused the nineteenth-century Philippine revolution. He evaluates their thoughts on the concept of man and society, the need for governmental authority and obedience to law, justification for revolution, problems of church and state, and the place of the executive and the legislature in the constitution. Majul attests to the intellectual stature of Philippine ideologues and their concern with the fundamental question of liberty versus authority. His excellent book will serve Philippine specialists and those unaware of Asian nationalist ideologies.

Alfonso studies Theodore Roosevelt's policy in the Philippines primarily as president (pp. ix-x). As assistant secretary of the navy in 1897 Roosevelt was involved in the Philippine conquest, campaigned for its annexation as vice-president, and as president became an indefinite retentionist. As Alfonso points out, Roosevelt heavily relied on the judgment of William Taft

and Elihu Root, whose racially prejudiced and imperialist views on the Philippines are extensively cited. But Alfonso, in his otherwise detailed study, ignores Roosevelt's important debt to the British imperialist, Rudyard Kipling, and his calculated attempts to twist Rizal's writings on liberty in order to oppose Philippine demands for independence. Roosevelt instituted the official policy to glorify Rizal and downgrade revolutionaries like Aguinaldo and Mabini, as pointed out by Usha Mahajani (*Philippine Nationalism: External Challenge and Filipino Response, 1565-1946* [1971], pp. 208-09, 243-45, 304).

Stanley's larger, controversial work deals briefly with the Spanish period and the American conquest and then substantially with the Philippine demands for independence and American attempts at indefinite retention, except for Filipinization under Wilson. The book stops at 1921 when a stalemate was reached as Harding, "a mild imperialist," reverted to indefinite retention (pp. 258-59, 262). Stanley also cites imperialist views of American ruling authorities, but, in referring to Dean Worcester's retentionist attempts and Philippine expertise (pp. 63, 208), he ignores the crucial point, noted by others (Mahajani, *Philippine Nationalism*, pp. 246-47, 256-58, 311, 342), that more than anyone else, Worcester used his anthropological knowledge to create a false image of Philippine backwardness thus giving a "scientific" reinforcement to imperialism. Filipinos bitterly hated him.

Stanley claims that the Hispano-Philippine society and culture were "disrupted and dysfunctional" in 1898, that American imperialism was "ambivalent and domestically vulnerable" (p. 265), and that "the peculiar character of Philippine history prior to the American conquest—specifically, the partial stultification of social, cultural and economic life by the Spaniards—and the nature of the United States as industrially the most developed and psychologically the most ambivalent of imperial powers gave the Fil-American encounter an explicitly developmental and consensual aspect" (p. vi).

Stanley's account of the much-discussed Philippine-American relations after the conquest is well researched, illuminating, and in places original. But his main thesis is questionable. Philippine society by 1898 had been molded into a nation with a sophisticated culture and well-educated leaders, brilliant ideologues, and politicized masses united in a revolutionary struggle, as is pointed out in several well-known studies by Teodoro Agoncillo, Cesar

Majul, and recently by Mahajani whose work Stanley is unaware of. He blurs over the protracted Philippine-American armed conflict and Philippine resistance that lasted through 1911-12 and during which 300,000 Filipinos were put in concentration camps; over 200,000 Filipinos died fighting or under an American "pacification campaign" and atrocities that far outweighed Filipino brutalities. As Agoncillo, Mahajani, Jame Blount (briefly mentioned by Stanley, pp. 209, 287n), and others have shown, it was military conquest, not a policy of attraction that established American rule, and there was nothing ambivalent about American imperialism. The policy of ruling through native aristocracy, *illustrados*, characterizes all forms of Western colonialism. As for the developmental nature of American rule, all colonialisms entail economic development of agricultural colonies into economic dependencies of the industrial ruling powers.

Actually, Stanley's book refutes his own thesis. The ruthless assertion of legitimacy of imperialism and American sovereignty by the retentionists (pp. 86, 101-02, 107-09, 163, 177, 185-89, 199) refutes the thesis of ambivalent imperialism. The failure of "imperialism of suasion" (pp. 265-78) reveals the illusory nature of Philippine-American consensus. What Stanley calls "nation-building" efforts through education, road building, and other measures in the hope that the Filipinos would accept the status quo (pp. 81-86, 111) were systematic attempts to destroy Philippine nationhood and apply technology of thought control. Stanley himself admits that military suppression, the imperialism of suasion, and attempts to "evade independence and co-opt politics" failed against the "unexpected depth and resilience of Philippine nationalism" (pp. 272-78). Ironically, the imperialism of suasion won in 1946 when the Filipinos accepted "independence" that was tantamount to indefinite retention by the United States.

USHA MAHAJANI

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CRAUFORD D. W. GOODWIN. *The Image of Australia: British Perception of the Australian Economy from the Eighteenth to the Twentieth Century*. Durham N.C.: Duke University Press, for the Duke University Commonwealth-Studies Center. 1974. Pp. xx, 255. \$8.75.

Although Australia cannot boast of observers in the class of Tocqueville and Lord Bryce, it has attracted many writers, some skillful—

like Sir Charles Dilke, Anthony Trollope, James Anthony Froude, and Sidney and Beatrice Webb—and others less able. Professor Goodwin has selected many Britons and a few Australians and presented their impressions of the Australian economy from the landing of the convicts to the present. ("Australian economy" is misleading because the writers usually discussed society as a whole.) Goodwin distinguishes between those who actually visited Australia and those, like Marx and Engels, who never left the distant Northern Hemisphere. Goodwin tells us how information about the Australian economy was brought to Great Britain, what stimulated the inquiry of the interpreters, and which bodies of social and economic theory they utilized.

"What did observers see or think they saw in Australian society," asks Goodwin, "and what did they convey to their audience?" Comment, in general, was unsophisticated and dwelt on the ordinary. Many, for example, were surprised that Australia could produce gold and wool simultaneously. Goodwin is less concerned with the accuracy or reasonableness of the observers' perceptions of the Australian economy than how an outsider reported what he saw, no matter how distorted the image. Goodwin concentrates on periods of change—the impact of the wool trade, the gold rush, the boom of the 1880s, the depression of the 1890s, and the Great Depression of 1929-34. Opinions about the Australian economy within British government offices (especially the Colonial Office and the Treasury) are for the most part excluded because historians have already given them attention.

British writers debated the question whether penal transportation was an effective economic or moral form of punishment. There was no more thorough indictment than that of Richard Whately, archbishop of Dublin, in 1832. Malthus and Ricardo debated the desirability of colonization with Robert Southey, the poet, and others attacked the wool trade. The immediate financial reaction to the gold discoveries was to argue the quantity theory of money, and rampant inflation was predicted. Marx and Engels were gloomy because the gold rush would bring new markets, a home for emigrants, opportunities for investment, and hence a delay in any revolution!

Travelers visiting Australia between 1860 and the 1880s had differing reactions to the squatters, to the reluctance of the Australian colonist to import nonwhite labor, to the boom of the 1880s, to the growth of the cities, and to the

imposition of protective tariffs. The child-parent metaphor was most frequently used to illustrate the relationship between Australia and England, the child using bad judgment leading to the panic of 1893. Commentators on the liberal economic experiments of the 1890s and early 1900s drew mixed conclusions. Nor could they agree on the cause of the panic of 1893: some blamed the tariffs, some the labor unions, and others excessive public works construction. The author minimizes the visit of Sidney and Beatrice Webb, whose impressions of Australia were distinctly unfavorable.

The final section of the book compresses too much material: economic experiments in price regulations, old-age pensions, labor arbitration, and child welfare legislation. Australia is often praised as extravagantly for her performance in the depression of the 1930s as she was condemned for her acts in that of the 1890s. In brief, she is no longer thought of as an economic heretic. The final chapter, beginning with 1914 and coming to the present, is short because Goodwin claims the material is relatively thin and repetitive. I wonder. Had Britain lost much of its curiosity about Australia? His concluding generalizations need re-examination and amplification.

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UNITED STATES

RICHARD M. HUBER. *The American Idea of Success*. New York: McGraw-Hill Book Company. 1971. Pp. x, 563. \$10.00.

One might well ask whether another book on American success is really necessary; there are so many and they all seem to say much the same thing about how we have always equated success with the accumulation of wealth. This lengthy history of the idea of success is not much different from the others in that respect, but it does make several distinctive contributions to the intellectual history of the success motif that render it valuable.

In surveying early American thought about success, Huber routinely presents vignettes of the usual success types such as Benjamin Franklin, Timothy Shay Arthur, Horatio Alger, and Andrew Carnegie, as well as some lesser used ones such as Elbert Hubbard (*A Message to Garcia*) and Lyman Abbott, but it is when he treats the twentieth-century salesman of suc-

cess like Bruce Barton, B. C. Forbes, Orison Swett Marden, Dale Carnegie, and Norman Vincent Peale that his work takes on a noticeable freshness and importance. His portraits of these purveyors (parasites?) of *How to Succeedism* are well drawn; their thought is clearly delineated and their influence effectively assessed. Here Huber adds some new material, if not totally fresh insight, to the serious reconsideration now under way of the social and intellectual history of the twenties and thirties.

Equally significant is Huber's explanation of the changing national mode of success from one rooted to a character ethic, wherein the idea stood solidly for what was morally good, to the present model, closely associated with the cultivation of personality and the mastering of techniques for handling and manipulating others. While acknowledging the importance of both the diminished influence of traditional religion on the American life-style and the impersonality, confusion, and affluence of twentieth-century urban society in bringing about that change, he believes that a mind power ethic ("think success brings success") that evolved from the New Thought movement of the late nineteenth century provided the necessary transitional thinking for the conversion of our society to its self-centered, completely materialistic conception of success.

Undoubtedly there is much in this book to take issue with, but, everything considered, it is a sound and suggestive work that merits attention. What we need now is a comparable study of "failure" in our national experience.

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WALTER I. TRATTNER. *From Poor Law to Welfare State: A History of Social Welfare in America*. New York: Free Press. 1974. Pp. xii, 276. \$9.00.

From Poor Law to Welfare State is Walter Trattner's effort to synthesize American social welfare history, to give social workers a sense of tradition, and most important, to resolve the frustration of teaching American social welfare history without a "textlike" book available. Trattner makes no claim to originality—"there is little here that is new," his preface tells us. Rather, he has tried to summarize the existing literature for students entering the field.

Let me quickly note that Trattner's overview does not result in a novel line of interpretation; those slightly acquainted with the literature need not examine this. And I am not certain

that practicing social workers will profit from the volume; it is too textlike to make for interesting reading. The only fair question to put to the book is whether those teaching social welfare history courses will find it useful to assign.

Certainly Trattner has followed the established guidelines for text writing. The book is brief, it has an upbeat tone, progress is at the core of the story, and every strong assertion is immediately followed by a qualifying paragraph. The writing is suffused with "still," "nevertheless," and "in any event." As a result, the book lacks for flavor. It will not outrage, excite, or engage students. But it will give them in capsule form highlights of developments in social welfare history.

Trattner's commitment to an optimistic even-handedness turns into formula writing; however mixed the motives of those who carried the banner of do-goodism, they ultimately furthered the course of social welfare in the United States. Thus he acknowledges Sydney James's interpretation of Quaker colonial philanthropy—not so much humanitarian as concerned with internal group solidarity—and then adds: "Be that as it may, the Quakers . . . whatever their motives, proved enormously helpful in times of stress" (p. 33). Or the founders of the New York Association for Improving the Condition of the Poor "no doubt loved the poor less than they feared or even hated them" (p. 64). But "in any event," they helped lay the groundwork for better understanding the "relationship between dependency and the environment" (p. 66). The Charity Organization Societies of the late nineteenth century made a fetish of organizational rationality, defining the problem of poverty more in terms of fraud, inefficiency, and duplicity than economic dislocations. Still, Trattner notes, they "ultimately fostered the development of a broader point of view," one that saw "social and economic causes of poverty as more pressing than personal inadequacy" (p. 91).

Trattner acknowledges the vast abuses of discretion in the operation of the juvenile court, that Progressive innovation. The 1967 Supreme Court *Gault* decision ostensibly remedied that, however, and "in any event . . . the principle of prevention was implicit in the juvenile court movement" (p. 111). True, the Widow Pension acts did maintain traditional distinctions between the worthy and the unworthy poor: "nevertheless, the statutes represented a major step forward," enabling "many families to remain intact who otherwise would not have

been able to do so" (p. 189). Many families? The program was administered in so niggardly a fashion that "a handful" would be the more accurate description.

In the end, the text is too bland for my tastes. I am not persuaded that progress is at the heart of social welfare history, and I would have preferred a book that took a tougher stance toward the incredible gaps between rhetoric and reality. Or, for that matter, I could in good faith assign a consistent exploration of the field along lines more congenial to Edward Banfield, where the poor seem more to blame for their own misery. At least that might have stimulated some vigorous student reactions. But others may well disagree here, preferring to let the text do the compromising while their lectures do the interpreting. For them, Trattner's volume will serve well.

DAVID J. ROTHMAN
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CHARLES F. CARROLL. *The Timber Economy of Puritan New England*. Providence: Brown University Press. 1973. Pp. xiii, 221. \$12.50.

There have been several recent studies of various aspects of the American timber industry, but this admirable little volume goes far beyond them in the subjects it discusses. It is limited pretty much to northern New England in the first century of settlement, but, whereas the others were concerned largely with forest products, Carroll branches out into an original and stimulating discussion of the impact of the forest upon the settlers and then of the settlers upon the forest.

At the outset, forest covered some ninety-five per cent of New England's area, with only a few openings where settlement might be easy. The forest primeval had a rather scary effect upon the newcomers—almost impenetrable in places and a haunt of strange beasts. It was only when the easier, open places were taken up that the trees themselves were tackled.

The opening chapter, "The Timber Shortage in England," has a doubtful relationship to the story. It shows the very widespread role of wood in the economy of 1600, with even firewood being very scarce. One would expect that here was a natural outlet for the American woodlands, but, except for naval masts, England imported very little wood from the colonies. The answer lay in freight charges, which bulked large in the cost of timber. Timber of good quality was available from Danzig, Memel, and other ports of the Baltic at an average of

some nine shillings a ton; from America, the rate was some 108 shillings. So the colonists' chief offering was scorned.

Other outlets were gradually found; first, white oak staves were shipped to Madeira and the Canaries for wine casks. The great beginning followed the introduction of sugar into Barbados and the other sugar islands after 1650. The cutting of pine boards for the West Indies produced a major onslaught on the forests of northern New England. There the story became a political-economic invasion of the pine regions, with the big timber merchants of Massachusetts invading Maine and, for a while, New Hampshire; there were religious undertones as well, for the men of the region wanted no Calvinist theocracy.

The forest primeval lasted just about a century, by which time over half a million acres of woodland had been cleared for farming. New England, of course, was not prime farmland—every field was full of rocks. There was some industry, notably shipbuilding, which consumed considerable amounts of wood. Cooperage and tanning also became important. In the more settled regions there was talk of conservation of trees, but to the north in Maine, which had the sobriquet of the Pine Tree State, where there were still plenty of pines, the slashing would continue well into the nineteenth century.

There are two useful appendixes. One introduces some useful analysis on shipping tonnage and the timber trade. The other contains six tables of departures from the port of Boston between 1662 and 1717. In every table Barbados has a long lead over the nearest competitor with nearly one-third of the tonnage, while almost another third was shipped to the other sugar islands.

ROBERT G. ALBION
Harvard University

BROOKS MATHER KELLEY. *Yale: A History*. (The Yale Scene. University Series, 3.) New Haven: Yale University Press. 1974. Pp. xi, 588. \$17.50.

Brooks Mather Kelley, former university archivist at Yale, has written this century's first complete overview of the history of Yale. It is, in most ways, a fairly conventional institutional college history that proceeds presidential administration by presidential administration. Most informative, suggestive, and beguiling is the one chapter that deviates from this pattern to discuss the rise of the extracurriculum, in which we discover how closely undergraduate

life at Yale emulated the competitiveness of the contemporary business world and how Yale's great unofficial football coach, Walter Camp, depended on his wife's (she was William Graham Sumner's sister) observations of daily football practice—he held a regular daytime job—in directing the team to victory after victory. But this history concentrates essentially on administrators, on their relations with trustees, alumni, governments, and faculty; on changing strategies for coordinating the constituent parts of the university; and on the growth and perpetuation of the "Yale spirit." Yale's intellectual history is relatively neglected.

This volume makes a splendid case for the often forgotten fact that Yale was an extremely innovative institution in the mid-nineteenth century under Theodore Dwight Woolsey (1846–71). Yale broadened its liberal arts curriculum to include requirements in the social sciences, modern history, modern languages, and science; offered a nonclassical undergraduate degree through its new scientific school; and granted the first earned Ph.D. degree in the United States (1861). In a wise and interesting assessment Kelley portrays A. Whitney Griswold—the volume ends with Griswold's death in 1963—as both the instigator of Yale's "march to greatness" and a somewhat narrow, private, self-righteous, and stubborn man who perhaps too vigorously sought to impose his ideals on Yale.

Kelley's narrative concentrates on Yale's administrative, organizational, and financial defects, yet his conclusions constantly reiterate that Yale has been and is "great." He uses the term "greatness" much too vaguely, and his understanding of the processes by which a university might achieve it seems incomplete. He appears at times to equate greatness with subordinating the college to the university; yet surely one of the reasons Yale remains a distinguished university is that it has never allowed the needs of graduate and professional education to overshadow its undergraduate mission.

Kelley seeks throughout to place the university in its social context. Unfortunately he has too little space to do this successfully and leaves us instead with a number of nagging questions about how Yale related to surrounding communities. What valuation of collegiate education inspired Saybrook's residents in 1718 to hide the college's library to prevent Yale from moving to New Haven? What exactly were the needs of the late nineteenth century that Yale's conservative curriculum failed to meet? Kelley believes alumni pressure on Yale since the Civil

War to have been more significant than other historians have thought, but he fails to describe how the changing postcollege experiences of Yale alumni may have influenced their demands. But in recognizing the influence of extrauniversity groups on the development of the university, Kelley helps alert us to how much more we need to know before we can fully understand the development of one of the most carefully studied institutions of higher learning in America.

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DAVID FREEMAN HAWKE. *Paine*. New York: Harper and Row, 1974. Pp. x, 500. \$15.00.

David Hawke regards Thomas Paine as a hedgehog. In politics, it is true, Paine knew only one big thing: the folly of monarchies and the wisdom of republics. In religion, too, he had essentially only one point to make: the irrationality of Christianity and the sweet reasonableness of deism. Temperamentally, however, Paine was, on Hawke's own accounting, more of a fox than a hedgehog: by turns self-centered and considerate of others, indolent and industrious, amiable and vindictive, independent-minded and eager of acceptance, shrewd and impractical, puritanical and convivial, generous but something of a free loader. In the end, though, as we finish this study of Paine's stormy career, we think of other words: indomitable, indestructible, unflappable, unsinkable.

Hawke's Paine is no wild-eyed radical. Though early to propose American independence, quick to support the French Revolution, and eager to promote republican revolutions in England and elsewhere, he had, as Hawke makes clear, a conservative strain in his thinking. He was sensitive to the plight of plain people everywhere but enjoyed associating with the high and mighty in America and Europe. He opposed property qualifications for voting but respected property rights. He favored equality of opportunity but did not expect equality of results. And the radical turn the French Revolution took following the execution of Louis XVI left him disenchanted. Hawke thinks he was really over his head in France.

Paine's great contribution, Hawke believes, lay in journalism. He wrote about complex social and political ideas with "elegant simplicity" and made them available for the first time to the average citizen. He also introduced the personal report into journalism, reported contemporary events with verve and was pre-

scient at times about the trend of events. Primarily a journalistic gadfly with a genius for dramatizing great issues and coining memorable phrases, he offended governing elites wherever he went. Robespierre's comment on newsmen like Paine: "We ought to proscribe those writers as the most dangerous enemies of the country, and to circulate an abundance of good literature."

Hawke's biography is admirable in every respect: perceptive in the use of sources, felicitously written, critical but compassionate, and careful to view Paine in the context of time and place. The effort to economize with footnotes is the book's only failure. By omitting superior numbers from the text and identifying the sources only of quotations, Hawke has made it difficult to trace some of the references he makes.

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JOHN A. NEUENSCHWANDER. *The Middle Colonies and the Coming of the American Revolution*. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press, 1973. Pp. 273. \$12.50.

Dealing with the period from the Coercive Acts to the Declaration of Independence, Professor Neuenschwander's book argues that New York, Pennsylvania, New Jersey, and Delaware were led to greater sectional awareness by the threat of independence and opposed a complete break with Britain from motives characteristic of their section. The author discusses the efforts made by Middle Colony leaders in the Continental Congress and the colonial assemblies and provincial conventions to avoid casting loose from the empire.

The thesis lacks sufficient evidence. The Middle Colonies' leaders did not commonly name their section, as Neuenschwander claims is characteristic of the "second stage" of sectional awareness. The author twice quotes Joseph Reed in mention of the Middle Colonies or provinces, but no other leader from the four colonies is cited.

The Middle Colonies opposed independence, according to Neuenschwander, because their leaders distrusted New England's "imperialism," had prospered within the empire, and possessed a "nabob mentality"—believing that their colonies would come to control the empire when it matured. Documentation of these motives is sparse. The author notes the hostility toward

New England manifested by Middle Colony leaders, such as John Dickinson, and the opinions of New England leaders toward the colonies to the south. Yet Dickinson was not appealing only to the Middle Colonies, and only one New Englander is quoted referring specifically to the middle provinces. Neuenschwander mentions that Maryland, North Carolina, and South Carolina voted with the Middle Colonies against important moves toward independence. A comparison of the motives of all these colonies might well be drawn here, in light of John M. Head's view that culturally heterogeneous colonies with rapid economic development opposed independence.

The reader will want more discussion and evidence. More is needed on the strength and influence of the Tories. Older, less significant classifications of the factional affiliation of delegates to the Continental Congress are reproduced; the more appropriate classification of John M. Head, in *A Time to Rend*, is only summarized in one sentence. The material on politics in the four colonies, especially some from unpublished Delaware sources, seems more useful than the discussions of the activities of the Continental Congress, which focus too narrowly on the issue of independence.

BENJAMIN H. NEWCOMB
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NATHAN MILLER. *Sea of Glory: The Continental Navy Fights for Independence, 1775-1783*. New York: David McKay Company. 1974. Pp. xi. 558. \$12.95.

This book is the first general history of naval operations in the American Revolution to appear since Gardner W. Allen and Alfred Thayer Mahan published their works in 1913. In spite of its subtitle, the book covers far more than simply "The Continental Navy Fights for Independence, 1775-1783," for it includes operations of the British and French navies, privateers, and some (not all) of the state navies. In twenty-five chapters, it covers all the major events from the Grenville plan for naval enforcement of customs regulations in 1763 to Rodney's victory over de Grasse in the Battle of the Saintes in 1782, plus many tidbits about life in an eighteenth-century man-of-war. It includes sixteen pages of good illustrations.

This book is obviously written for the general reader. It generally fills this purpose well, though it contains too many minor factual errors and misleading statements, such as a discussion of the layout of a ship of the line in

a chapter about the ships of the Continental Navy, whose largest vessels were frigates. The writing is vivid, if occasionally overblown, and usually fun to read.

For the scholar, however, this is a very frustrating book. Many articles and monographs about aspects of the naval side of the American Revolution have been written since 1913, and Miller has incorporated many of them in digested form. His knowledge of the mechanics of citation and bibliography, however, is imperfect to say the least. Many scholars who read this book will find chunks of their own work, slightly paraphrased, without benefit of footnote, while others receive full, almost effusive, credit. The bibliography is just as inconsistent. Some, but not most, periodicals have volume numbers, and only a few of the multi-volume works are so cited. Nowhere, either in footnotes or bibliography, is a manuscript source mentioned, although there is a hint in the introduction that some may have been used. Certainly one would expect a Washington-based writer at least to have looked at the transcripts of British Admirals' dispatches in the Library of Congress.

This is a decent book for the popular reading shelf, better than most pop books on the Revolution. The definitive naval history of the American Revolution, however, must await completion of the *Naval Records of the American Revolution*, whose six fat volumes to date have not yet reached 1777.

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E. JAMES FERGUSON *et al.*, editors. *The Papers of Robert Morris, 1781-1784*. Volume 1, February 7-July 31, 1781. [Pittsburgh:] University of Pittsburgh Press. 1973. Pp. xlix. 437. \$17.50.

To historians of Beardian cast, Robert Morris (1734-1806) is usually depicted as a sort of evil genius, the manipulator of men, money, and the machinery of government in ways somehow not quite clean. Indeed the eastern Calvinist contingent of the Confederation Congress saw him in that character even then. E. James Ferguson, the principal editor of the Morris papers, manifests this ambivalent attitude toward a man he terms "except for George Washington, the nation's most influential leader," from 1781 through 1783. "As a result of his leadership, the component elements of the nationalist movement were united behind a coherent program, which combined constitutional change with economic reform." But, says Ferguson, Morris's

program "appealed primarily to the classes of the population that had wealth and status." But despite his mixture of awe and distrust of Morris, no historian better understands the fiscal history of the Revolutionary and Confederation periods than Ferguson. His *Power of the Purse* (1961) stands as a monument of clarity in a forest of obscurity.

This volume exhibits the public Morris—little personal correspondence has been found and none is included—as he negotiates the unprecedented authority over public affairs and governmental management he demanded, attempts to separate his private and public relationships, and establishes himself as the director of not only the finances of the United States but of Pennsylvania as well. The sweep of his authority was no greater, however, than that of his plans for the new nation. Morris aimed, he told John Jay, "to unite the several States more closely together in one general Money Connection, and indissolubly to attach many powerful Individuals to the Cause of our Country by the strong Principle of Self Love, and the immediate Sense of private interest" (p. 287). Morris comes across as a man of great energy, tremendous administrative ability, unequalled knowledge of economic, fiscal, and commercial principles and practice, and even of what passed for integrity in the eighteenth-century world of commerce he inhabited.

Ferguson and his staff have chosen to limit publication to those papers relating to Morris's activities as superintendent of finances, a position he held officially from February 20, 1781, to November 1, 1784. The chronological limitations are wise: they make possible the expeditious publication of the important public papers of this earliest and highest of nationalists. Exception can be taken, however, to other criteria of selection. Since the most controversial aspect of Morris's career was his mixing of private and public business, individual scholarly judgment would be facilitated if the editors had been thoroughly inclusive. Why, for instance, print a note from John Jay wholly concerned with family affairs (p. 98), but omit one from a Morris partner in private business, Thomas Mumford, that concludes, "I am putting all my business out of my own Management and care in order to make an Effort to Serve the Public" (p. 76)? If saving space was a factor, a number of public documents might well have been omitted. Should a ten-page report by Charles Thompson (pp. 193–202) already in print in the *Journals of the Continental Congress* or a report on the military hospitals by

James Tilton (pp. 323–37) published as a pamphlet in 1813 be reprinted here?

The major shortcoming of the work, however, is no fault of the editors, who have made every possible effort to locate each of Morris's widely scattered bits of public correspondence. But though reproducing or excerpting some 152 letters, 57 diary entries, and 20 other documents, the editors are forced to guess at the contents of 107 "letters not found." In addition, the footnotes carry allusion to scores more in this category. Thus one is often as much intrigued as informed by this collection. Nevertheless, in the hands of this team of skilled and extraordinarily knowledgeable historians the series will help clarify the question of how the United States solved the most complex problem it ever faced—how to finance a war without an income.

Without attempting an exhaustive exposition of the context of each document, the editorial apparatus is a model of thoroughness and clarity. The index is adequate and accurate, though the Morris entry should be more thoughtfully subdivided. The series will be published at the rate of one or two volumes a year, and it will comprise about sixteen when completed.

CHRISTOPHER COLLIER
University of Bridgeport

DOLORES A. GUNNERSON. *The Jicarilla Apaches: A Study in Survival*. DeKalb: Northern Illinois University Press. 1974. Pp. xv, 326. \$12.50.

Lacking a dramatic history of confrontation with the government, the Jicarilla Apaches have received only passing attention from historians and only slightly more from anthropologists, but as anthropologist Dolores Gunnerson demonstrates, they have an extremely interesting past. Utilizing the tools of archeology, ethnography, and history, Gunnerson has attempted to piece together an ethnohistorical account of the Jicarillas during the Spanish period in the Southwest that emphasizes the gradual amalgamation of Apachean groups and also cultural survival.

Gunnerson postulates that the Cuartelejo and the Paloma Apaches, who lived in semisedentary farming villages while also relying heavily upon the buffalo for subsistence, came from the Dismal River archeological complex in the Central Plains. Pressure from enemy tribes caused them to join the Carlana Apaches and to migrate to the fringes of Spanish settlements in New Mexico where they were on friendly terms with

the Spaniards and the Pueblo Indians. Pressured by other tribes and also by Spanish desires to settle them around a mission, they drifted south to Texas and Coahuila where they were called Lipiyanes and Llaneros. Eventually Spanish activity against other Apache groups caused the Llaneros to return to New Mexico where by 1800 they claimed to be part of the Jicarillas who had remained in the mountains near Taos. The Mountain Jicarillas, despite close contact with the Pueblos and the Spaniards, demonstrated an ability to adapt to changing conditions but preserved the core of their culture.

Although the evidence is not conclusive, and undoubtedly some scholars will reject this approach, Gunnerson has presented a viable interpretation of Jicarilla history. Moreover, her study effectively relates the impact of other tribes, the influence of changing Spanish Indian policies caused by the threat of the Comanches and the international situation upon the bands that eventually became known as the Jicarillas. Although the organization of the book is occasionally somewhat awkward, and the author is too willing to include long quotations, this is a useful interpretation of Jicarilla history before 1800. It is an example, also, of the value of combining ethnographical and historical research.

RICHARD N. ELLIS
University of New Mexico

D. H. MEYER. *The Instructed Conscience: The Shaping of the American National Ethic*. Reprint; Philadelphia: University of Pennsylvania Press. 1974. Pp. xiv, 220. \$3.95.

There is probably no group in American philosophical and cultural history less well understood than the numerous minister-philosophers in America's burgeoning colleges during the first half of the nineteenth century. Meyer is knowledgeable about what has been written about this group, both by philosophers and historians, and adds his share to an understanding of its role and importance in American intellectual history.

Meyer considers a small subset of the group, the members of which, he thinks, as teachers and authors of textbooks in moral philosophy, were influential in shaping a national ethic. He mainly considers Francis Wayland, Asa Mahan, Charles Grandison Finney, Archibald Alexander, Francis Bowen, Mark Hopkins, Noah Porter, and James McCosh. He realizes that theologically and even ethically this subgroup

is a hodgepodge but believes that the members of the group had these important characteristics in common: they taught moral philosophy and wrote textbooks and as a group helped shape (or reflect, or both?) an isomorph of Victorian morality in the United States. In America, as in Britain, this Victorian moralism amounted to a secularization of religious piety and devotion and acted as a bridge between the wholly theological ethics of earlier years and the completely secularized ethics of later years.

Meyer's generalizations are necessarily quite general, since the members of his subgroup have so little in common. (Strictly speaking, they did not even have textbook writing in moral philosophy in common, since Finney's moral philosophy appeared in his *Systematic Theology*.) Even though Meyer's large-scale generalizations have value, they are not really as interesting as the issues that divided these people, knowledge of which gives us the real flavor of the age. Happily Meyer's book contains some analyses of this sort, but more detailed ones are needed. Meyer covers too many topics and issues for a short book. He neatly contrasts Alexander's Old Light views on determinism with the New Light and Unitarian views of other members of the group and also contrasts clearly the intuitionism of Wayland and Mahan with the teleological views of Finney. But we need to know more about this Edwards-Finney teleological view than Meyer tells us. Is it really a variant of utilitarianism, as Mahan argued vigorously, or is it neither wholly teleological nor deontological but a mixture of both? The latter is true, I believe; it is teleological because "right" is derived from "good," but it is deontological, because acts are not right or wrong independent of intentions. Meyer discusses Oberlin "Perfection," which is fine, but fails to distinguish between the Finney-Cochran view and Mahan's view, which was Wesleyan in nature and accounted for his eventual switch from the Congregational to the Methodist Church.

EDWARD H. MADDEN
State University of New York,
Buffalo

JAMES F. HOPKINS *et al.*, editors. *The Papers of Henry Clay*. Volume 5, *Secretary of State, 1826*. [Lexington:] University Press of Kentucky. 1973. Pp. viii, 1096. \$20.00.

Throughout 1826 Clay's correspondence remains predominantly optimistic, yet the year

was for him one of frustration. The Panama Congress, from which he had high hopes of hemispheric unity, met with bitter partisan and sectional opposition and led to a bloodless duel with John Randolph. Out of it, however, came what is perhaps Clay's greatest state paper, his detailed instructions to the U.S. commissioners. While the secretary's energies turned toward Latin America and domestic politics, relations with Great Britain deteriorated visibly, although Clay seems still unperturbed when he accepted Rufus King's resignation as minister in May. His instructions to King's successor, Albert Gallatin, reviewing all points at issue between the two countries, is another state paper of prime importance, though, again, its major objectives—settlement of the Northeast boundary dispute, free navigation of the St. Lawrence, and the opening of the British West Indies to American trade—were not realized. The activities of the maladroit Poinsett in Mexico provided yet another problem for the secretary, while the long controversy with France over spoliation claims was just beginning.

On the domestic front Clay continued throughout this second year of the Adams administration to act as though he were in fact an unofficial prime minister and party leader. In the interest of harmony he rejected the old labels of Federalist and Jeffersonian Republican, arguing that there existed only two parties—the Administration and the Opposition, more popularly National Republican and Jacksonian or Democratic. The party he strove to build was basically conservative, favoring a strong central government and tending to rely on the alliance of East and West that had secured Adams's election. He continued to dispense patronage with a view to perpetuating the administration and strengthening the National Republicans at state and local levels, but it was not enough. The defection of Amos Kendall's Kentucky *Argus*, which began attacking Adams in the fall, and lost its public printing in consequence, was surely a straw in the wind, but there is little indication in these papers that Clay realized the extent of his defeat. He continued to believe, to the end of the year, that his newly forged party was winning the off-year elections, when in fact it was being soundly beaten. He seems not to have realized that the old New England Federalists, with their merchant constituency, were not yet ready to embrace the tariff, while the old Jeffersonians still distrusted Adams.

The editors continue and expand upon the practice introduced in the preceding volume of abstracting or summarizing many routine documents, and their judgment as to what is to be printed in full is excellent. Like its predecessors, this volume is rich in source materials for the period covered, illuminating not only the conduct of foreign relations but also the inner workings and strivings of the Adams administration.

CHARLES M. WILTSE
Dartmouth College

RUSSEL BLAINE NYE. *Society and Culture in America, 1830-1860*. (The New American Nation Series.) New York: Harper and Row. 1974. Pp. xiv, 432. \$10.00.

Can one write a cultural history of the United States for the years 1830-60 and pay little or no attention to such customary topics as democracy and the common man, the aggressive pursuit of gain, the distribution of wealth, manners, the home, city life and rapid urbanization, the routines of farm life, even Andrew Jackson? Russel B. Nye has accomplished this near miracle in his most recent publication. His book deserves appraisal, however, not on what he has left out but rather on what he has set out to do.

Faced with a thirty-year time period and the overwhelming abundance of amorphous material comprehended under social and cultural history, Nye seeks to give it meaningful form in his book by focusing attention on the "development of [four] key American ideas and institutions": nationalism, the sense of mission, romanticism, and progress. Along with these central ideas he stresses the rage for reform—except in the South—directing attention especially to peace, poverty and vice, prison reform, the care of the insane, temperance, the status of women, utopian experiments, and slavery. All this Nye develops interestingly and persuasively in his first two chapters.

The remainder of the book is devoted to topics usually covered under cultural history. Some, like the three chapters devoted to literature and art—nearly one-third of the book—do not always fit as nicely into the author's frame of reference as the reader—and probably the author, too—would wish. For the most part the ground covered in this area is familiar territory, but the treatment, though breaking little new ground, appears surprisingly fresh and up to date. Of course in summarizing so much ma-

terial over three decades, the writer can hardly avoid some passages that read like an annotated bibliography—a pitfall that through skillful handling Nye ordinarily, though not invariably, avoids. Of the other topics treated I found such sections as those on medicine and technology well-executed summaries of material not readily obtainable elsewhere.

Along with his earlier volume *Cultural Life of the New Nation, 1776-1830* (1960), Nye has now completed a remarkably fine guide to American civilization covering more than eighty years of our cultural history. In this recent book he does not hesitate to reach back to the earlier years or to look forward beyond 1860 when some advantage is to be gained. On the whole this is the useful, workmanlike product expected from this seasoned scholar.

GEORGE ROGERS TAYLOR
Amherst College

HOWARD MUMFORD JONES. *Revolution and Romanticism*. Cambridge, Mass.: Belknap Press of Harvard University Press. 1974. Pp. viii, 487. \$15.00.

This book forms the central panel of a triptych between *O Strange New World: American Culture—The Formative Years* (1964) and *The Age of Energy: Varieties of American Experience, 1865-1915* (1971). All three works have the vast perspectives and the humane foreground detail of the Hudson River school landscapes, of which Professor Jones has written with such sympathy. Like the first two, this latest volume is perhaps, as he says, "not for specialists," but both the specialist and the general reader will find here good writing and stimulation.

This trio of studies has purposed to examine "the complex problem of the relationships in art and thought between the New World, more particularly the United States, and the Old." "The essential matter of history," Jones postulated in *O Strange New World*, "is not what happened but what people thought and said about it"—in this case, the operative ideas that have guided and expressed Western cultural development, particularly in the United States. The exploration ranges over a wide landscape. *O Strange New World* examined from 1492 to about 1830 the relationships of diverse European intellectual and artistic movements to both of the Americas. *The Age of Energy* investigated many aspects of American culture between 1863 and 1915 besides its dynamism. And the present volume, encompassing, with necessarily some overlapping, the years 1763 to

1861, goes beyond its title, for the first 150 pages establish a point of departure in a definition of eighteenth-century society and culture, the Enlightenment, sensibility, and neoclassicism. The investigation spans the Atlantic and passes beyond the history of ideas to political, social, and economic history, to the history of philosophy, religion, literature and the other arts, to analyses of individual writings, and to a good deal of intellectual biography.

Inevitably even so learned and encyclopedic a scholar as Professor Jones cannot hope to control rigorously all the details of so vast a panorama. The specialist may find incidental factual errors (Rousseau's *Confessions*, part 1, was published not four years before his death [p. 235], but four years after); he may be surprised at failures to refer to certain secondary studies (as an authoritative interpreter of the French Revolution, should not R. R. Palmer be cited for his monumental *Age of Democratic Revolution* rather than for his earlier *Twelve Who Ruled* [p. 299]?); and he may wish for qualifications of some of the necessarily summary interpretations of certain writers.

Such lapses are not serious, however, for Jones aims to interpret rather than to be authoritative or exhaustive. The essential value of the book lies in its articulation of the two great simultaneous cultural phenomena, revolution and romanticism. The latter he does not attempt to define in its totality, but he finds its essential characteristic the expression of "a new formula for the individual—*le moi romantique*." He quite rightly, I think, sees that one runs into a "confusing chronological and thematic puzzle" if one tries to define the relationship between the two in "linear," that is, causal or chronological, terms. True, "there is a frequent connection between the revolutionary spirit and the spirit of romanticism." Yet romanticism, as identified, predated revolution in the Rousseauism of the 1760s, and the American Revolution, and likewise the French in its initial and final stages, was dominated by leaders certainly representative of the rationalistic "high eighteenth century." Counterrevolutionaries like Chateaubriand were romantics. Clearly, "there can be revolutions without romanticism and romanticism with revolution." Jones's point, if I read him accurately, is that it was romanticism that provided the justification of revolution: "There cannot be citizenship unless there is man, and the rights of man is a phrase essentially without meaning unless one believes, as the ro-

mantics did, that each human being is more than the totality of his own history."

DURAND ECHEVERRIA
Brown University

CAROL E. HOFFECKER. *Wilmington, Delaware: Portrait of an Industrial City, 1830-1910*. [Charlottesville:] University Press of Virginia, for the Eleutherian Mills-Hagley Foundation. 1974. Pp. xvi, 187.

Carol E. Hoffecker, assistant professor of history at the University of Delaware, has written a concise and interesting account of the industrialization of Wilmington, Delaware.

Urban historians often state that the process of urbanization is accompanied by declining community spirit, with negative results upon social institutions. Professor Hoffecker's thesis is that this position is not always true. She concludes that as the city of Wilmington became urbanized, its communal and social institutions were enhanced. The author believes that Wilmington's business leaders were instrumental in fostering a communal spirit.

After a brief introduction telling of Wilmington's founding and growth in the seventeenth, eighteenth, and early nineteenth centuries, the author starts her analysis in 1830 when Wilmington was a grain processing and distribution center. At that time Wilmington had a weak municipal government and few social institutions. Industrialization transformed the city, however. Its social fabric was changed. Independent artisans and merchants no longer dominated the town. Instead, factory workers became the largest occupational category in Wilmington's economy.

From 1840 until 1900 Wilmington's economy was predominantly industrial, focused around railroad-car construction, shipbuilding, carriage making, and iron founding. During this period the people of Wilmington organized many public and private institutions such as the public school system, churches, and charitable institutions. Municipal services, such as the building of parks and sewers, improved living conditions.

Wilmington was fortunate in having its wealthy families remain within the city and not flee to the suburbs. This forced them into a commitment to improve the city's quality of life and helped foster a spirit of cooperation that made nineteenth-century Wilmington a liveable industrial city. The city was also blessed in that strikes and labor violence were rare.

The book's main weakness is the omission of

urban politics during the period studied. However, the high-quality illustrations, the author's lucid style, and her interesting thesis make this a valuable contribution to the field of urban history.

ROBERT J. WECHMAN
New School for Social Research

WALTER M. MERRILL, editor. *The Letters of William Lloyd Garrison. Volume 3, No Union with Slaveholders, 1841-1849*. Cambridge, Mass.: Belknap Press of Harvard University Press. 1973. Pp. xxiii, 719. \$30.00.

With this volume we now have approximately half the Garrison letters in published form. This in itself is welcome. Moreover, Walter Merrill has done an extraordinarily valuable job of editing. He and his staff have indefatigably sought out information on all of Garrison's references to names, meetings, events, and Biblical and literary passages.

The editorial notes are largely informative rather than interpretive, even more so than the first two volumes; and this allows Garrison to stand on his own without too much subjective commentary. In addition the notes are a fine biographical guide to the antislavery movement. (Incidentally, an analysis of the men and women abolitionists mentioned in Garrison's letters further discredits the status-dislocation thesis popular a decade ago.) It would have been much more work for the editor, but the collection would have been measurably improved with the addition of a subject index and at several junctures condensed versions of the letters to which Garrison was replying.

There are 287 letters, all (outside of the appendix) in chronological order. Yet they fit relatively well into five general categories, which Merrill introduces with short essays. The years 1841-49 saw Garrison struggling to maintain the American Anti-Slavery Society after damaging schisms, formulating the idea of disunion, traveling on his British Mission, followed closely by a mission to the western American states, and involved in the anti-Sabbath controversy.

Unfortunately the essays are unimaginative and sometimes misleading. Merrill's contention that Garrison's lack of interest in the Northampton Community, even while visiting there, "demonstrated clearly that he was a man of action, not a social philosopher" (p. 119), is an example of a single-case generalization, is contradicted by most of Garrison's career, including letters in this collection, and is a *non*

sequitur. Merrill also implies that nonresistance was an "extraneous reform" in relation to Garrison's "ultra views on abolition" (p. 540). Lewis Perry has indeed shown that Garrison and many of his followers were sometimes confused about whether they were pursuing abolitionism as a secular reform or as part of a millennial nonresistance movement, but there was always an intimate, organic connection between the two ideas. This is certainly true for the period covered by these letters. Garrison wrote, for example, on October 1, 1844, "I rejoice . . . that the cause of non-resistance is gaining new adherents continually, through the radical character of the anti-slavery movement." And throughout the 1840s Garrison's strictures against the Liberty party were at least partially based on the fact that political abolitionism was a form of coercion, and thus it was a violation of nonresistance.

In any case the letters here are well edited and relatively unencumbered, and readers will be able to make their own judgments.

GERALD SORIN

State University of New York,
College at New Paltz

ROBERT E. MAY. *The Southern Dream of a Caribbean Empire, 1854-1861*. Baton Rouge: Louisiana State University Press. 1973. Pp. x, 286. \$10.00.

Although the origins of the Civil War in slavery economics and politics have been vigorously raked over in recent years, the aggravation of the sectional conflict by promoters of pro and antislavery foreign policies continued to be neglected. Surely the time must come when general histories, especially college texts, will give, if not equal space, at least fair place to the roots of disunion in the North-South split over foreign policy during the fifties.

The resentment of many Southerners against the failure of the United States government to annex all of Mexico, Nicaragua, and especially Cuba was more bitter than historians have realized. Robert E. May's book proves that. He explores the width and depth of Southern desire to turn American expansion southward and to take territories that would add political power to the slave states and enrich the Southern economy. He finds interesting evidence of minority opposition to this program even in the South, and he pays attention to the crucial refusal of the Lincoln Republicans to compromise their opposition in the winter of 1860-61, when the only alternative was secession.

May has done most with the kind of material that is hardest to explore—obscure as well as famous Southern newspapers—which he oddly fails to list in his bibliography. These and the many collections of personal papers he searched prove that the hope for a tropical empire was a large component of the Southern drive for power inside or outside the Union.

That is the chief argument of the book. A secondary argument is that the program of southward expansion was widespread nationally until 1854 when it became almost exclusively Southern. Aiming to prove that Manifest Destiny became what he calls "Sectional Destiny" in that year, the author minimizes Southern interest in Cuban annexation, which probably dominated the movement, from the Treaty of Guadalupe to 1854. He describes how Narciso López turned to the South for support of his filibuster expeditions to Cuba but does not explore the proslavery interests of John L. O'Sullivan, the New York leader of the filibusters. *La Verdad*, the Spanish-English newspaper of the proslavery Cuban exiles published in New York beginning in 1848, is not mentioned.

Conversely, May undervalues his own account of Northern commercial interests in Nicaragua after 1854. He is convincing on the point that after William Walker, as dictator of Nicaragua, quarreled with Cornelius Vanderbilt, his restoration of slavery in Nicaragua was calculated to win Southern support for his ventures. But he argues that the many Northern Democratic politicians who supported southward expansion did not support extension of slavery into the Caribbean islands and Central America. Here he is on shaky ground. In Cuba, above all, the Freeport Doctrine meant perpetuation of slavery in case of annexation, and Stephen A. Douglas always supported annexation of Cuba.

These arguments by May suit his wish to confine proslavery foreign policy rather strictly to the South. As in the fight over expansion of slavery inside the Union, so in the conflict over foreign policy, Southern militants had the support of important Northern leaders and newspapers. May's evidence that some Northern Democrats avoided tropical expansion when it became "too entwined with the issue of slavery" (p. 180) is not as impressive as, for example, Stephen A. Douglas's—he of the Freeport Doctrine—proposing to Alexander Stephens, after Lincoln's election, that Mexico enter the Union as a slave state (surely he meant more than one slave state) if the South agreed not to secede.

The trouble is that slavery embarrassed all

but the toughest fire-eaters most of the time and their Northern friends just about all the time. One private admission of the proslavery motive balances and overbalances a hundred public protestations of other, more pleasant motives.

Narratives of the American moves to grab off tropical territories between the Mexican War and the Civil War are in the general histories of Allan Nevins and others. May has done a great deal to work out the meanings of the strange story. For that he deserves full credit. It is no discredit to his valuable book to say that more remains to be done.

BASIL RAUCH
Barnard College,
Columbia University

GERALD S. HENIG. *Henry Winter Davis: Antebellum and Civil War Congressman from Maryland*. New York: Twayne Publishers. 1973. Pp. 332. \$7.50.

Based in part on newly discovered papers in the Samuel F. du Pont Collection, Professor Henig has written a commendable, definitive biography of Maryland's tempestuous congressman, Henry Winter Davis, who was a Whig, a Know-Nothing, a Bell Democrat, and finally, a Republican Unconditional Unionist. Davis blamed the Civil War on slave-owning, aristocratic Democrats but he refused to join the Republicans until after the war started. He sought a position in Lincoln's cabinet and blamed his unsuccessful attempt, as well as his failure to regain a congressional seat until 1863, on his hated Maryland rival, former Democrat Montgomery Blair.

There are some surprising disappointments in the book. Henig never fully explains Davis's intense, growing hostility toward Lincoln, although the latter wooed him with patronage and in private talks. Curiously, Henig never mentions that Davis chaired the important Committee on Rebellious States, nor does he recount the Marylander's efforts to undermine the Navy Department as part of the executive-legislature struggle. Most disappointing is his failure to provide an adequate background for the Wade-Davis Bill and Davis's use of the Constitutional guarantee of a republican government in the states as Congress's attempt to offset Lincoln's military reconstruction. He also leaves doubts as to why the Wade-Davis Manifesto proved to be so disastrous to the advocates of black equality and civilian control of Southern occupation.

Henig, however, dissects Davis in a concise, well-written book that contributes to our understanding of Maryland politics, Republican party leadership, and the mixed motivation of an opportunistic, antislavery, border-state politician. Happily, he does not stereotype the Radicals or Davis. Henig portrays him as a powerful, unpredictable leader who feared that the Union would be restored as it had been.

Davis's death in 1865 leaves the reader with the same question as Lincoln's earlier assassination—how far would either man have been willing to go to impose a militarily won social revolution on a war-weary, confused people?

MARVIN R. CAIN
University of Missouri—
Rolla

D. P. CROOK. *The North, the South, and the Powers, 1861-1865*. New York: John Wiley and Sons. 1974. Pp. x, 405. \$11.95.

Interest in American Civil War diplomacy has in recent times produced so many specialized analyses and perceptive interpretations that the time seemed ripe for a synthesis of them. D. P. Crook, an Australian historian, not only accomplishes this feat, he also focuses more sharply than has ever been done before on the international ramifications of this war. He is primarily concerned with the interplay between the powers, particularly Great Britain, France, and Russia, and the long-term consequences of the ultimate fate of the Federal Union as seen by their contemporary leaders. Though agreeing with the prevailing view that Great Britain played the key role among the major powers concerned with the future of America, an integral part in its international economic system for the promotion of Britain's prosperity, Crook does not underestimate the important effect of potential Russo-American cooperation on the policies of France and England.

In his masterful synthesis the author illustrates how the cause of the Union benefited from "the unprincipled egotism that is the soul of European diplomacy," as well as from certain coincidental developments in the world at large. Significantly, the Civil War coincided with major power shifts in Europe and with far-reaching advances in naval technology, matters of vital concern to all powers. Crook develops these concerns and their impact in the context of Atlantic history. His discussion of all aspects that affected the policy decisions of

the powers gives new meaning to many well-known facts.

In his overall perspective of the Civil War and its consequences, Crook comes to the conclusion that as a result of Europe's uncertain "neutrality" during this war America "hastened the process of economic and cultural disengagement" from Europe. Its relationship with Britain and France in the decades following the Civil War reflected the strains caused by the maritime powers during the war. But while the end of the war reawakened America's sense of "Manifest Destiny and Mission," ironically, Congress and the public lost their appetite for expansionist policies, and the successful defense of republican institutions in the United States did not give heightened impetus to republican sentiments in Europe. Indeed the legacy of this war to European politics was of very limited scope. This stimulating scholarly book deserves to be read widely.

HENRY BLUMENTHAL
Rutgers University

DAVID EDWIN HARRELL, JR. *The Social Sources of Division in the Disciples of Christ, 1865-1900.* (A Social History of the Disciples of Christ, volume 2.) Atlanta: Publishing Systems. 1973. Pp. xi, 458. \$19.95.

Serving as a sequel to a similar volume on the antebellum period, this book seeks to develop an account of "what Disciples thought and did about social issues" (p. ix) from 1865 to 1900. Organizing his material into broad topics that fall within the designated time period, some of the chapters seem quite conventional in subject matter—the response of Disciples to urbanization, organized labor, the Social Gospel—while others delve much more enticingly into such topics as black Disciples, pacifism, and law and order. Harrell's book is important in part because it discusses a denomination that embraced both North and South and reflected the social complexities of two still very divergent sections of the country. He sees divisions created by the Civil War still serving as extremely powerful determinants in the lives of Disciples in the postwar period. He also views class distinctions as central to any understanding of the response of Disciples to pressing social issues. Indeed it is the emerging middle-class, liberal outlook of increasing numbers of Disciples that seems at the heart of many social tensions in the denomination at the end of the century. Thus according to Harrell, sociological factors, not religious norms, determined ulti-

mately how most Disciples responded to industrialism and an ever-increasing secularism in American society. Almost three decades ago Henry May, in his *Protestant Churches and Industrial America*, sketched a similar conclusion, but Harrell's comprehensive and detailed analysis provides much new and interesting corroborative evidence at the denominational level.

I feel, however, that Harrell has downplayed far too much theological and specifically religious emphases as influences on the social attitudes of Disciples. There is little or no discussion of the theological debates that accompanied the rise of the Social Gospel and served as a primary justification for increasingly liberalized social attitudes for a large minority of Disciples. Moreover, a consideration of the author's subject within the broader constraints of evangelical Protestantism would have made his discussion of Disciples' attitudes toward "benevolence" and the special problem of "morality" (both major concerns of all evangelicals) much more meaningful.

One also senses all too often the need for deeper analysis and summation of data to accompany and perhaps even occasionally to replace the frequently long narrative and factually oriented passages of the book. The last chapter serves effectively as a broad summary and overview, but similar analytical passages would enhance other sections (chs. 10 and 12, for example) where more specific topics are considered. In any case, Harrell's solid though somewhat plodding study provides us with all the detailed information we now need to confirm again the capture of American Protestant Christianity by the surrounding secular culture in the late nineteenth century.

JAMES FINDLAY
University of Rhode Island

ROBERT M. UTLEY. *Frontier Regulars: The United States Army and the Indian, 1866-1891.* (The Wars of the United States.) New York: Macmillan Publishing Company. 1973. Pp. xv, 462. \$12.95.

In his second contribution to the Wars of the United States series, Robert Utley completes the descriptive narrative of the frontier army. In *The Sword of the Republic*, Francis Prucha traced frontier military activities from 1783 to 1846; in *Frontiersmen in Blue*, Utley carried the story through the Civil War; and in this volume, he treats the last twenty-five years of frontier warfare.

Writing in dispassionate tones, the author is concerned primarily with strategy, specific tac-

tics, and engagements. By including short, but incisive descriptions of military organization, weapons, equipment, and military life on the frontier, he meets the requirements of traditional military history. While Utley introduces no new themes and refrains from embellishing older ones, he adds fresh insight into the workings of a "peacetime" army at war.

William Tecumseh Sherman's postwar army was beset by a host of internal and external problems that served to frustrate both planning and performance. Describing the efforts to overcome them, Utley displays considerable organizational and writing skill. His assessment of the army strikes a balance between the heroic stereotype urged by the military and the "ugly" stereotype framed by nineteenth-century humanitarians. In his objective appraisal, Utley finds that the frontier army was an admixture of "wisdom and stupidity, humanity and barbarism, selfless dedication and mindless indifference, achievement and failure, triumph and tragedy." He suggests that the soldiers were neither "agents of empire" nor a "barbaric band of butchers." Rather, he portrays the frontier army as a "conventional military force trying to control, by conventional military methods, a people that did not behave like a conventional enemy and, indeed, quite often was not an enemy at all."

Controversial interpretations are not avoided, but inexplicably, Utley failed to add a concluding chapter wherein he might have explored more fully some of the major implications of frontier warfare. The rationale that military actions were prompted by impulses to civilize and not to exterminate is sound, but the descriptions of many campaigns suggest that the process came dangerously close to the "destroy to save" attitudes that surfaced in our most recent military conflict. The concept of total war, with attendant killing of women and children, is not excused by Utley, but it is rationalized beyond both reason and the author's evidence. The additional chapter would have allowed him to buttress his claim that the army was becoming more professional and to examine the transitional nature of the closing years of his study. Too, many of the problems present in 1866 were still with the army when it entered its conventional war with Spain, and a summary account of them would have been helpful.

While this well-written and thoroughly researched book contains much evidence that will be familiar to scholars in the field, all will welcome the marshalling of that evidence in

one book. Both the breadth and depth of Utley's research are impressive, and his ability to make coherent the many disparate elements that constituted military action against, and national policy toward, the Indian will be appreciated by scholars and lay readers alike.

H. DUANE HAMPTON

University of Montana

LAURA WOOD ROPER. *FLO: A Biography of Frederick Law Olmsted*. Baltimore: Johns Hopkins University Press. 1973. Pp. xvii, 555. \$15.00.

It has been half a century since a major biography of Frederick Law Olmsted, Sr. has appeared. This superb new work sheds light where the old one (by F. L. Olmsted, Jr. and Theodore Kimball) is least satisfactory—the development of Olmsted's personal and professional life in the years before he became a great landscape architect. Mrs. Roper was given complete access to the rich collection of Olmsted family papers (now deposited in the Library of Congress). Several highly professional analyses of Olmsted's landscape designs have appeared recently, and Roper wisely chose not to go over that same ground again. *FLO* is a more personal work. It presents in striking detail Olmsted's many careers and interests that connect him with reform movements from the 1840s to the 1890s. From an early age Olmsted regarded himself as a social philosopher and reformer; but it took him forty-two years to settle on landscape architecture as the ultimate expression of that commitment.

Born in Hartford in 1822 and raised in very comfortable surroundings, the young Olmsted was restless and ill trained. Fortunately his father often financed and always encouraged young Frederick through abortive careers as a merchant, farmer, journalist, editor, public administrator (for the U.S. Sanitary Commission), and business manager. While friends like Charles Loring Brace moved directly into reformist vocations, Olmsted was unable and/or unwilling to commit himself to a single career. His appointment to the Central Park superintendency came by pure chance, and he tried two more jobs before returning full time to landscape work.

Career fluidity being common in the nineteenth century, Olmsted did not suffer by shifting from one profession to another. An intelligent and hard-driving man, he built a solid record of achievement that kept opening new doors. Whether traveling in the Old South as a reporter, negotiating publishing contracts

in England, or managing a gold mine on the California frontier, Olmsted was a sharp observer of the physical and human environment. Over the years he developed a reflexive sense of order and organization in all that he saw and heard. Artistic sensitivity aside, he was a brilliant administrator and perceptive politician—traits that proved essential in turning “gardening” into the respected profession of landscape architecture.

No previous work has so clearly detailed the thirty-four-year struggle waged by Olmsted to defend the integrity of the Central Park Plan against the encroachments of New York politicians (Tammany bosses and reformers alike). Like most landscape architects, Olmsted sadly reported that his plans were “constantly and everywhere arrested, wrenched, mangled and misused”; but he never lost faith in America or became cynical of mankind. He was sustained in difficult times by his warm family relationships. With his office in his home, Olmsted was aided by his wife and two of his sons. While Olmsted was continually harassed by health problems and saddened by the untimely deaths of several of those closest to him, the sustaining and humane fabric of his family always remained strong.

In sum, Frederick Law Olmsted emerges as the embodiment of the best public and private traditions of nineteenth-century America. That he was not even more influential as a landscape architect is a tangible loss to our environment. This magnificent biography helps recapture the life of a man who is himself as beautiful and noble as the landscapes he created.

JOSEPH L. ARNOLD

University of Maryland Baltimore County

R. HAL WILLIAMS. *The Democratic Party and California Politics, 1880-1896*. Stanford: Stanford University Press. 1973. Pp. x, 290. \$10.00.

In the political history of California the eighties and nineties were a breather between the hammering out of a new constitution in 1878-79 and the grand house cleaning triggered by the Lincoln-Roosevelt League in 1907-11. That reform drive reinforced the traditional preference for Republican governors. Within living memory the only exceptions were Culbert Olson and Pat Brown. With the eighties the compendious histories of Hubert Howe Bancroft and Theodore Hittell ran down and out. Also the essence of the period appeared to be in economic improvement and growth, a fabulous real estate boom, a rush of healthseekers,

a competing railroad, and burgeoning schools and colleges. Not surprisingly we have waited this long for a study in depth of the Democrats of 1880 and 1896.

In his study of the California Democracy, 1880-96, R. Hal Williams found useful collections of papers; newspaper files, particularly the San Francisco *Examiner* and *Chronicle* and the Los Angeles *Times*; a few monographs; and a substantial number of scholarly papers, several of them digests of doctoral dissertations. On the national context he used the Cleveland and Harrison papers and a much longer shelf of monographs.

As of 1880 he finds the antimonopoly Democrats led by an improbable trio: George Hearst, owner of mines and the state's foremost Democratic paper; Christopher Buckley, boss of the dominant city; and Stephen M. White, an aspiring politician in Los Angeles. In 1882 this faction won the governorship, but with Republican help the conservative Democrats organized the state senate and elected a Republican to the United States Senate. The liberals were further embarrassed when the new Railroad Commission failed to regulate railroad rates. A drive to collect proportionate taxes from the railroad also failed. By withholding all payments for three years the railroad forced an ignominious compromise. Worse yet, California litigation produced the Supreme Court ruling that corporations as “persons” had Fourteenth Amendment protection.

Even the election of a Democratic president did little for the California Democrats. Unable to locate the power center of the antimonopolists or the conservatives, Cleveland made appointments that satisfied neither faction. He also was insensitive to the California fixation on Chinese exclusion. He made a political correction in time for the 1888 campaign, but so did the Republican candidate. In his second term Cleveland offended the California Democrats by refusing to annex Hawaii and by intervening to break the railroad strike of 1894. Californians were much more interested in defeating the funding bill, which would have written off most of the federal loans for the construction of the Pacific Railroad.

Throughout these sixteen years the California Democrats continued discordant and divided. Their best achievement was in sending a capable and constructive senator to Washington, Stephen M. White. They argued reform needs that Hiram Johnson and company would push through in 1911, but their actual political achievements were mediocre. Williams

provides a neat and useful account of this activity with particular excellence in relating it to that of the nation.

JOHN CAUGHEY
University of California,
Los Angeles

JOHN E. DIMEGLIO. *Vaudeville U.S.A.* Bowling Green, Ohio: Bowling Green University Popular Press. 1973. Pp. 259. Cloth \$8.95, paper \$3.95.

It is easy to envy a man who owns an inscribed photo of Joan Blondell, has conducted a personal interview with Mae West (tape recorder off), and whose grandfather—a vaudeville magician—taught him a new trick every Sunday. It is a pleasure to thank John DiMeglio for sharing not only his heritage but also the fascinating observations patiently elicited from Ben Blue, Ted Lewis, Ken Murray, and others equally central to this subject. There is much to be savored here. The best part—not surprisingly—comes through that hard-honed, soft-tough patois developed by the seasoned performer and used here to depict the life of the three-a-day troubador.

DiMeglio does not stop with pure nostalgia but offers vaudeville as a symbol of American traits ranging from free enterprise to the pioneer spirit. In spite of these grand assertions, however, the true scope of this work is not “vaudeville USA,” but those circuits developed by a handful of Manhattan impresarios between the 1890s and the 1930s. Thus he gives but passing notice to burlesque and the minstrel show, both vital to his larger thesis but outside his actual focus. More often, however, the work fails by introducing topics too complex and numerous for this short text.

Contradictions abound; some need not be resolved, but all need clearer definition. Was vaudeville typified by its variety and independence or by its uniformity of bookings and assemblage? Did the performer represent America by the life he led on stage or off? Most crucial is the contradiction between using vaudeville as *direct* evidence of social concerns or, since DiMeglio often states that the show served as a release from social tensions, using it as *inverse* evidence.

An important example pertains to a subject the author is inclined to skirt: that preponderance of humor based on ridicule of minority groups. If an audience laughs uproariously at the fate of a stage fool done in dialect, will it then—purged of prejudicial feel-

ings—leave the theater more inclined toward social justice? Or is the laughter more precisely what it seems? A related matter is DiMeglio's stress on vaudeville's self-censorship, which he tests mainly on grounds of hidden meanings and exposed skin. It seems doubtful that demeaning aspects of ethnic humor received much serious self-policing.

The author shows that there is much entertainment left in vaudeville and, for the historian, much utility. How to use it, fairly and precisely, without removing all its warmth and color is still an open question.

ROBERT H. WALKER
George Washington University

BARBARA STUHLER. *Ten Men of Minnesota and American Foreign Policy, 1898–1968*. (Minnesota Historical Society, Public Affairs Center Publications.) St. Paul: the Society. 1973. Pp. xii, 263. \$8.50.

History may well be what has been thought, said, and done by mankind, but written history is, always, a job of packaging. Here we have an ingenious package: an illumination of United States foreign policy through the vehicle of incisive and intelligent sketches of ten politicians from a single state. The result is first class because of ingenuity, scholarship, writing style, and the exhibition of respectful concern for the right to individuality among these sons of Minnesota. This is “state and local history” with national and international significance.

Impressed with “the integrity and strength of the Minnesota political experience,” the author—professor and associate director of the World Affairs Center at the University of Minnesota—has written biographical sketches of figures who attained national identification with foreign affairs issues. Cushman K. Davis illustrates early imperialism; Charles A. Lindbergh, Sr., Harold Knutson, and Henrik Shipstead illuminate isolationism; Frank B. Kellogg, Joseph H. Ball, Harold E. Stassen, and Walter H. Judd reflect the postisolationist period; while Hubert H. Humphrey and Eugene J. McCarthy illustrate clashes on internationalism and interventionism in the Vietnam era.

The author does not claim that one must understand Minnesota to understand America, nor was there a unique interaction; but, she hopes, there is illumination of “the shifting national attitudes of reluctance or enthusiasm toward American involvement in world affairs” during a seventy-year period. The ideas dis-

played include "conservative and liberal Republicanism, third-party radicalism, and a progressive Democratic party orientation."

The chapter headings offer interesting capsules: empire marched in step with Davis; Lindbergh was a radical isolationist; Kellogg engaged in impassioned diplomacy; Ball pioneered in internationalism; Stassen searched for peace (and office); and Judd had a "mission."

One wonders if his own state could sustain a "ten men of" book like this. If undertaken, would it prove fundamentally different? Ethnic differences might be substantial—Minnesota in 1930 was "predominantly rural" with parentage 23 per cent German, 19 per cent Swedish, 18.8 per cent Norwegian. Differences in state productivity (cotton, automobiles, silver, or sugar come to mind!) might well change patterns. Differences in states as presidential springboards, or one-party orientations, could prove meaningful. A regional rather than state base might offer advantages.

Books such as this one (and T. A. Larson's *Wyoming's War Years* [1954]) can be good teaching vehicles, so paperback editions would have utility. Professor Stuhler's book offers, fortunately, adequate definition of terms and identification of issues to be self-explanatory in student hands. And the calm prose will help keep the classroom a place for careful consideration of conflicting ideas, a place where localism may be merged with globalism and state concerns may be seen in a setting of national problems.

VAUGHN DAVIS BORNET
Southern Oregon College

JERRY ISRAEL. *Progressivism and the Open Door: America and China, 1905-1921*. [Pittsburgh:] University of Pittsburgh Press. 1971. Pp. xxiv, 222. \$9.95.

In 1953 the British historians John Gallagher and Ronald Robinson called attention to "the imperialism of free trade," thus challenging the more common belief that mid-Victorian England had reversed its imperialist course in favor of the fairer policy of encouraging international free trade. What was "fair," inquired Gallagher and Robinson, when England's unique economic power assured it of dominating influence wherever it intruded? In American historiography the Open Door has served revisionists in comparable fashion. Although George Kennan, in his remarkable little book, *American Diplomacy, 1900-1950* (1951), found John Hay's Open Door assertions symbolic of Americans' naive, idealistic approach to foreign

relations, other critics of U.S. foreign policy have detected more sophisticated, not to say cynical, intentions. "We do not need to seek an unfair advantage," the *Nation* editorialized in 1901. "An open door and no favor infallibly means for the United States . . . the greater share and gain in the commercial exploitation of China" (quoted by Israel, p. 13). Precisely! say the revisionists. Given America's enormous economic power, to expect other nations—rival powers and host countries alike—to compete without using what political leverage they possessed in itself implied giving the Americans unfair advantage.

In this book Jerry Israel, associate professor of history at Northern Illinois, attempts to squeeze the complicated story of U.S. relations with China into a "competition" versus "cooperation" framework. "Competition" means the Open Door; "cooperation" means consortium-type ventures and the League of Nations. Israel hopes to show that American China policy was only an extension of the Progressive reform program, which also vacillated between cooperation and competition ("New Freedom" versus "New Nationalism"), and which also was designed to reduce the likelihood of domestic radicalism. "Canton, like Chicago, was to be cleansed of crime and corruption. China was to be remade in the American image" (p. vi). Despite uncertainties and inconsistencies in tactics, says Israel, overall strategy remained the same: namely, "the Americanization of China." This strategy was foredoomed because American industrialism and American ideas about moral reform were inapplicable to China; and insofar as domestic progressivism depended on Americanizing the world, that too was foredoomed.

Israel thus continues the debate about whether the Open Door policy was an expression of benign interest in China's welfare or whether it was on the contrary a subtle though potent instrument of U.S. determination to share in the spoils. But his organizing scheme does not work very well. He strains for a formula that would bring China policy into the Williams-Kolko framework of aggressive capitalist corporation. His efforts to cram the interesting material he has gathered quickly becomes tiresome and bewildering, especially when it is plain that the word "cooperation" appears in diverse contexts with obviously contrasting meanings. And since the same individuals evidently held both "competitive" and "cooperative" ideas simultaneously, one may wonder how those categories can be useful.

Surely American leaders did apply comparable principles to domestic and foreign problems, but to express the relationship in New Freedom-New Nationalism terms highlights only an analogue, not a true parallel, and offers little toward clarity and comprehension. Maybe it would be better to acknowledge that U.S. policy makers have, from the beginning, sought advantages for Americans and have used various, even contradictory, techniques for the purpose. "No one was more thoroughly amazed than Hay," Robert E. Osgood wrote twenty years ago in *Ideals and Self-Interest in America's Foreign Relations* (1953), that anyone should have seriously taken the Open Door "pronouncement as a principle of beneficence."

Israel does a fine job of demonstrating that American policy makers no more regarded China as a sovereign nation with which the United States could deal as an equal than did any other world power. That said, it must also be noted that China's political disorganization and economic weakness left the country exposed to foreign predators who, as the game of international politics was played, had no reason to acknowledge moral obligations to the prey. In a general way, the Open Door may be correctly understood as merely another technique for purchasing advantages for American interests, but in the immediate context of the Far Eastern situation, whatever John Hay's personal intentions, the Open Door did appear to express a unique concern for China's interests. That may not have been without some worthwhile influence at a time when imperialism still meant mostly conquest and extraterritorial truculence.

In spite of its faulty thematic structure, Israel has written an extremely interesting and informative book. He has used some inaccessible sources, such as the privately held Charles R. Crane and Martin Egan manuscripts. The student who persists past the distracting conceptual paraphernalia will be well rewarded.

RICHARD M. ABRAMS
University of California,
Berkeley

JOHN J. BROESAMLE. *William Gibbs McAdoo: A Passion for Change, 1863-1917*. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press, 1973. Pp. xii, 304. \$15.00.

This excellent study provides considerable insight into one of America's most influential cabinet officers, and it is a valuable addition to

a growing number of monographs probing the complex relationships between the Wilson administration and the business community. Broesamle's research is thorough, his writing is clear, and his narrative is interpretive.

The book focuses mainly on McAdoo's first years as secretary of the treasury, from 1913 to 1917, but includes a fascinating sketch of his rise from bitter poverty as a child in Georgia to financial success and renown as a major figure in the completion of the Hudson River tunnels. Broesamle shows that McAdoo was in many respects a study in paradox: an opponent of special interests who was nonetheless partial to farmers, especially Southern cotton producers; a warm and unassuming person who was "almost compulsive" in his search for prestige; and a successful entrepreneur who was temperamentally outside the broad spectrum of contemporary business thought and distrustful of business leadership.

McAdoo's dominant characteristic, however, was that of promoter, and it is in this context that Broesamle develops the theme of his "passion for change." Flexibility, an emphasis on action, and a deep-rooted faith in the American environment were basic to McAdoo, who brought an unfailing booster spirit to his work in and out of government. Although he supported numerous progressive causes—such as women's rights and child welfare—and although he spoke of "genuine up-lift" and the moral functions of government, it was McAdoo's concern for efficiency that pushed him beyond many of his contemporaries toward enlarged federal responsibilities. He ultimately concluded, in Broesamle's words, that "centralization had become essential in order to liberate the pent-up energy of capitalism itself" (p. 114).

Although pre-Keynesian, McAdoo was "remarkably innovative" in using government to free credit during periods of tight finance. He struggled also to open up Latin American markets so that America's economic expansion would be less dependent upon the continuation of war orders from Europe. By 1916 he even advocated government operation of industries when private capital lacked either the means or the will to act in the national interest. Broesamle sees McAdoo's proposed government-owned shipping company as symbolic of his "federal promotionalism"—using the public sector to help private enterprise—and the "inherent nationalism" of the Wilson administration. The author, who notes the irony and contradictions of much of this in light of the New Freedom rhetoric, ends his narrative with Mc-

Adoo's Treasury Department on the threshold of becoming "a great weapon of financial warfare." That part of the story also needs telling, and, one hopes, with results comparable to Broesamle's fine book.

LEROY ASHBY

Washington State University

JOAN HOFF WILSON. *Ideology and Economics: U.S. Relations with the Soviet Union, 1918-1933*. Columbia: University of Missouri Press. 1974. Pp. xv, 192. \$10.00.

Debate over the United States's delay in recognizing the Communist regime in Russia for sixteen years after the Bolshevik Revolution of 1917 has resulted in numerous and varied historical interpretations. In this study Joan Hoff Wilson examines some of these views, especially that the U.S. business community exercised a significant influence, and concludes that the role of business has been misinterpreted both in substance and effort. She suggests that despite constant ideological opposition to the Soviet regime within government circles, a growing number of businessmen, but still a minority within their community, were intrigued by the possibility of trading with the Soviets. They became increasingly indifferent to an ideological rationale for nonrecognition and consciously separated economic from political foreign policy in the case of the Soviet Union. Nevertheless, despite economic self-interest, they did not generally advocate recognition because of their awareness of the government's firm ideological commitment. Moreover, after 1923, U.S. businessmen participated in a relatively steady increase in trade with the Soviets. During the same period, Soviet experts within the State and Commerce departments refused to admit that this basic contradiction had developed and virtually ignored those businessmen who most strongly favored cementing economic relations. They also ignored the dramatic change in favor of recognition that developed in public opinion between March 1931 and October 1933. They did not change primarily because ideological opposition to the USSR had become institutionalized within the bureaucracy during the 1920s, despite the steady improvement in trade relations. Wilson suggests that it was in this way that U.S. foreign policy became as ideologically motivated as that of its most feared enemies. She stresses the view that the publicly organized business support for recognition that did exist came belatedly—after Roosevelt had decided to grant U.S. recognition of Russia be-

cause he feared the growing Japanese power in the Far East. As a result, neither the opinion of businessmen nor general public opinion influenced the process leading to recognition, despite the new administration's defending this diplomatic act as a measure to alleviate the depression.

Wilson has written a sophisticated, provocative, and well-documented study that is, however, vastly more effective in demonstrating the failure of the business community to influence America's ultimate decision to recognize the Soviet Union than it is in proving that Roosevelt's final decision was based primarily on political considerations.

BETTY M. UNTERBERGER

Texas A&M University

DAVID C. DEBOE *et al.* *Essays on American Foreign Policy*. Preface by LLERENA FRIEND. Introduction by ROBERT H. FERRELL. Edited by MARGARET F. MORRIS and SANDRA L. MYRES. (The Walter Prescott Webb Memorial Lectures: 8.) Austin: University of Texas Press, for the University of Texas at Arlington. 1974. Pp. 146. \$5.00.

This slim volume represents the proceedings of the eighth annual Walter Prescott Webb Memorial Lecture Series, held at the University of Texas at Arlington in March 1973. An affectionate memoir of Webb by his student and associate, Dr. Llerena Friend, precedes four lectures on disparate themes in recent American diplomacy. They are briefly introduced by Robert H. Ferrell, who firmly sets the essays within the boundaries of counterrevisionism.

David C. DeBoe writes of Henry L. Stimson's attempts to breathe life into the Kellogg-Briand Pact. DeBoe advances the thesis that Stimson forced his belief that the Pact obligated the United States "to consult" with the powers on a reluctant Hoover, without success. Subsequently he prevailed upon a compliant Roosevelt, who briefly sought to involve the United States at the Geneva Disarmament Conference during the Hundred Days, before capitulating to Senate isolationists. DeBoe's essay is logical but argued within such narrow limits that one must ask why consultation was so important to Stimson. This common tendency to concentrate on means and ignore ends inexorably leads to a focus on diplomatic process and a consequent neglect of objectives.

This limitation is much more apparent in the second essay, Van Mitchell Smith's "Africa: the Kennedy Years, 1961-1963." Culled largely from the works of New Frontiersmen, this thin

pastiche uncritically magnifies Kennedy's foreign policy accomplishments in coping with an un-integrated series of African crises. The author accepts pious declarations at face value, with posturings substituted for policies concretely rooted in American economic and geopolitical concerns. Much is made of the Kennedy charm and his commitment to "moderate" policies, none of which receive sophisticated definition.

Somewhat more successful, although based on a perilously thin research sample, is Elliott West's attack on the belief that the American press during the war painted roseate views of the Soviet Union. Some influential newspapers and magazines, he concludes, became quite disenchanted, as the war progressed, with misperceptions and impossibly high expectations making the cold war a historical inevitability. But were leading editors and commentators actually so unworldly as to assume the identity of Russian and American beliefs and objectives? Or might many of them have been patriotic propagandists for as long as it seemed useful?

The three resident faculty were joined by Norman A. Graebner for the final lecture, here reprinted as "Japan: Unanswered Challenge, 1931-1941." In his thoroughly professional manner, Graebner struggles with a paradox familiar to traditionalist diplomatic historians: assuming that the United States had no compelling reasons for protecting the status quo throughout East Asia, why did Washington insist on terms that left Japan with the alternatives of surrender or war? Graebner clearly understands the Japanese dilemma, that necessary industrial expansion meant forcing economic and political change in the Asian status quo. Most revisionist historians find conflict inherent in the controlling imperatives of the Open Door policy. For Graebner, however, the paradox: while acknowledging the American preoccupation with "the stability of the post-Versailles international order," he finds "the minimal interests" of the United States "scarcely worth the price of war." Thus Pearl Harbor came "by inadvertence."

These brief essays only cohere in their reaffirmation of the virtues of traditional diplomatic historiography. This is a minor encounter in the continuing range war among students of American foreign policy.

JIM WATTS
City College,
City University of New York

RICHARD J. BONNIE and CHARLES H. WHITEBREAD
II. *The Marihuana Conviction: A History of Marihuana Prohibition in the United States.*

Charlottesville: University Press of Virginia.
1974. Pp. xiv, 368. \$12.50.

Marijuana appeared in the United States, brought to the West by Mexicans and to New Orleans by West Indians soon after 1900, when middle-class Americans had come to fear both narcotics and ethnic minorities. American officialdom, mixing Mexican folklore about marijuana's prowess with antipathy to the drug habits of lower-class foreigners, took for granted that marijuana led to "addiction, lethargy, crime, insanity, and death." The Harrison Act increased fear of marijuana, deemed the drug of choice by addicts denied narcotics.

Thus caprice of time and circumstance falsely defined marijuana as a narcotic, considered by some more dangerous than heroin and cocaine. This picture of the national marijuana consensus emerges in this excellent study by two University of Virginia law professors, both of whom served with the National Commission on Marijuana and Drug Abuse. Judicious in tone, relying heavily on Bureau of Narcotics archives, Professors Bonnie and Whitebread expertly intertwine sociological, psychological, medical, political, and legal strands into a richly complex history of marijuana attitudes and control, a worthy companion to David Musto's first-rate history of narcotic control, *The American Disease*.

Flowing from worried consensus came state laws and the national Marijuana Tax Act of 1937, "a paradigm of the uncontroversial law," secured by a few pressure groups orchestrated by the Federal Bureau of Narcotics. The authors take pains not to make bureau director Harry J. Anslinger sole mover, though he appears as key leader and most frenzied spokesman. "If the hideous monster Frankenstein came face to face with the monster Marihuana," said Anslinger, "he would drop dead of fright."

A new plateau of McCarthyistic fear came in the 1950s, bolstered by the stepping-stone theory—that marijuana created an appetite for opium. Tougher laws increased penalties and reduced flexible judicial options. Rigid law and soaring use by the young finally cracked the long-standing marijuana consensus. The first sound research dispelled many marijuana myths, and public opinion retreated from making criminals of over two hundred thousand young people annually arrested for possession. Law remains rigorous, although enforcement has relaxed.

Besides describing these developments in careful detail, the authors prescribe what they be-

lieve proper policy should be, one recognizing hazards marijuana may possess but bringing relief to our "wounded" legal system.

JAMES HARVEY YOUNG
Emory University

JOANNE BOCK. *Pop Wiener: Naive Painter*. [Amherst:] University of Massachusetts Press. 1974. Pp. 157. \$20.00.

Appreciation of ingenuous, or naive painting has been fostered by collections like the Garbisch Collection in the National Gallery and the Abby Aldrich Rockefeller Collection in Williamsburg. Now we meet "Pop" Wiener, naive painter. Miss Bock first heard of him while studying in Cooperstown in the summer of 1969. Her book is the result of interviews with him in Monticello, New York.

Isidor Wiener left Romania when he was seventeen while Russian pogroms against Jews were in effect. He landed in New York in 1903. He worked hard always, but when he retired at sixty-five, death had claimed two of his three sons and his beloved wife. The future seemed bleak. It was then that Dan, his remaining son, gave him a set of watercolors from the ten-cent store and asked him to paint. Nothing in Wiener's life had prepared him for painting. From the first brush stroke, however, he felt a thrill of joy.

Evident in his art are overtones of embroidery and icons of his Romanian childhood. Delight in his adopted country is expressed in rural landscapes with many flowers and animals. There is rarely a cityscape. The complex rhythms of Manhattan were not for him as a painter. Nor are there vividly remembered experiences as in Horace Pippin. All is here and now, with the important exception of his Biblical subjects. In them the quiet pastoral mood can change to an energetic rush, as in the little masterpiece "Crossing of the Red Sea," with Egyptians on horseback plunging into the water and the children of Israel standing saved on the opposite shore.

Landscape is persistent in his work, often peopled with delightful small figures with large eyes like those in Assyrian bas-reliefs. In "Moses and the Ten Commandments" small Hasidic figures face a Moses many times larger, as in the scale shift in early Italian paintings of a saint and donors. There is a "Noah's Ark" with a mélange of birds and beasts, each particularized, the result of many visits to the zoo. "Daniel in the Lions' Den" shows a cage with a blue and white tiled floor and the lions

showing only token ferocity. Roseate walls are folded back to frame the den. In several little pastoral scenes color is so felicitous as to be melodic.

"Pop" Wiener died in 1970 leaving behind works with his own amalgam of qualities that charm. His painting is worth getting to know.

JOHN GERNAND
Washington, D.C.

JAMES O. YOUNG. *Black Writers of the Thirties*. Baton Rouge: Louisiana State University Press. 1973. Pp. xiii, 257. \$10.00.

The literary criticism of black literature, like that of most minority literature, has been burdened by a desire to speak supportively, to view the literature as a political and social expression of a "people," and therefore to blur idiosyncratic distinctions among individual writers. There has rarely been the security that allows for self-criticism. In recent years, however, it is clear that critics and scholars are approaching black history and literature with greater confidence. James O. Young's *Black Writers of the Thirties* is a fine example of this new criticism: it presents the past in objective, balanced terms.

In a comprehensive way, Mr. Young examines the crucial era of the 1930s, when art and politics met most critically at the crossroads of the depression, and tries to assess what writing has remained of permanent significance, what social action has proved to be most sensible in the course of time. Young makes a distinction between the older generation of historians, critics, and scholars—"race men," in the words of Horace Cayton—and the young radicals. A race man, in Cayton's words, was "an individual who was proud of his race and always tried to uphold it whether it was good or bad, right or wrong"; he was, as Young interprets the term, a man who subscribed to the values of nineteenth-century middle-class society and championed "thrift, hard work, sacrifice, respectability, and individual enterprise." The radicals thought of the race men as provincial, for they believed that "many of their problems were really the problems of labor, not necessarily of race." The older men concealed many of the difficulties confronted by black people, glorified racial traits, and portrayed respectable race heroes; the younger generation presented realistically the lives of black men and women and attempted to achieve in their work a universality of expression.

Although there was considerable variance among them, the race men included such figures

as Carter Woodson, the romantic historian who glorified the black past; Benjamin Brawley, the literary critic who exaggerated the merits of Negro literature; W. E. B. DuBois, the black chauvinist who despaired of America and became increasingly bitter in the thirties. These older race men believed in self-sacrifice and rugged individualism; their literary taste was genteel and distant to folk experiences; they spoke of what the black experience ought to be rather than what it was. The young intellectuals who emerged during this decade—Ralph Bunche, E. Franklin Frazier, and Abram Harris—were more concerned with economics than with civil rights. The problems confronting black workers, in their view, were those of class, not race, and they tried to see these questions in broad economic terms.

Young pits these two groups against each other in a rather facile way. His book is a panorama of almost all the significant black political scientists, sociologists, economists, poets, novelists, and essayists of the time, and because he discusses so many figures in so little space, he has a tendency to categorize writers rather than consider their complex personal nature. There is no question that he favors the young radicals who dominated Howard University in the thirties at the expense, for example, of a complex figure like W. E. B. DuBois. But his real admiration is reserved for those who, in his view, "were successful in their search for universality within the reality of black experience. Unlike their genteel and 'New Negro' predecessors, they saw no need to make their characters middle-class idealizations or primitive creatures of joy in order for them to be significant or interesting. Instead, they attempted to look objectively into the lives of the common black folk." In the practical world, A. Philip Randolph was most successful in asserting the rights of black people who belonged to the labor unions; in literary criticism, Sterling Brown was able to write histories of black literature that were sensitive and critical; in literary creation, Sterling Brown, Langston Hughes, Zora Neale Hurston, William Attaway and Richard Wright—especially Richard Wright—were most successful.

There is a slightly simplistic dialectic in this book: the romantic race men confronting the radical critics yield to the more balanced, complex, and universal authors who emerge at the end of the decade. But despite Young's tendency toward easy formulations, his study is a useful and comprehensive account of a most critical decade in black culture. His favorable

judgments of A. Philip Randolph, Sterling Brown, and Richard Wright seem justified by history; these men were indeed the leading and most powerful figures of this period, and their vision has proved to be far-reaching.

Young has read a vast amount of the scholarship in this period, and he uses it with ease and grace. His book is always interesting and should be a valuable resource for any student of the period.

THEODORE L. GROSS

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KEITH W. OLSON. *The G.I. Bill, the Veterans, and the Colleges*. [Lexington:] University Press of Kentucky. 1974. Pp. x, 139. \$9.25.

To the student of the history of higher education in America, this well-written little volume is of much value. The author points out, quite correctly, that secondary works in the field have been disappointing in their treatment of the Servicemen's Readjustment Act of 1944, and he attempts to fill the void.

In his efforts, the author gets off to a good start in chapter 1, where he has an unusual and candid account of the economic origins of the law. This view is quite in order, and historically sound, even though the most commonly held opinion is that the law was designed as repayment by a grateful nation for services rendered. It is appropriate, however, that we should be reminded that the framers of the law had a great fear of the social, political, and economic consequences of massive numbers of discharged soldiers without jobs, Mussolini's march on Rome, the rise of German fascism, the American labor unrest and violence of the early thirties—these evoked fear of the consequences of demobilization after World War II.

After the first chapter, the volume tapers off a bit in interest and content. Some of the accounts of new developments, such as the impact of married veterans on campus, are worthwhile, but one has the impression of a story being told in a drifting manner. And one wishes that so many of the changes mentioned by the author could have been brought together in a more systematic pattern. There is, for example, in chapter 4 one sentence on the rather important transformation of Rutgers University into the State University of New Jersey, but this is then followed by three pages devoted to an interesting discussion of the role of the GI Bill in bringing about the establishment of the State University of New York.

The last chapter, covering Korean and Vietnam benefits, either should have been greatly enlarged or should not have been a part of this volume.

The bibliographical note and seventeen pages of additional notes at the end add much to make this volume a welcome addition to the literature on the subject.

LYMAN B. BURBANK
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THOMAS M. CAMPBELL. *Masquerade Peace: America's UN Policy, 1944-1945*. Tallahassee: Florida State University Press; distrib. by University of Florida Press, Gainesville. 1973. Pp. vii, 226. \$12.00.

Much of this study was originally a dissertation, and the vexing problem of transforming a dissertation into an engaging book has only partially been overcome. *Masquerade Peace* is a clearly written product of thorough research, but it is tedious and slow moving.

The author, an associate professor of history at Florida State, concentrates on the shift in American United Nations policy during the early stages of the Truman administration. In so doing, he rejects Gar Alperovitz's thesis that Truman engaged chiefly in atomic diplomacy in order to make Russia more amenable to American postwar goals. According to Campbell, Truman placed more emphasis on the UN than on the atomic bomb. But the president's faith in the UN as a means of preserving American security had eroded as early as the San Francisco meeting. Thus from the summer of 1945 through the spring of 1946 Truman moved to substitute an Anglo-American police force for the more international approach of the UN. Campbell argues that UN policy under Truman became a deceptive mask or masquerade peace behind which the United States and Russia struggled for world supremacy while American leaders sought other security alternatives. The author presents a good case for his thesis, and although he rejects much of Alperovitz, Campbell concedes that atomic diplomacy was an integral part of Truman's foreign policy.

It is clear that the strongest asset of the book is the analysis of the controversy that arose between the United States and Latin America over the UN. Campbell skillfully reveals how Secretary of State Edward R. Stettinius, Jr. and others badly miscalculated Latin American intentions. At the Mexico City meeting (February 21-March 5, 1945) Latin American leaders refused to abandon the concept of regional

security for the international approach. Thus American diplomats found themselves in the awkward position of having to reconcile internationalism with historic guarantees of regional security. By postponing a decision on this and other issues, the Americans considered the meeting a success. But such postponement precipitated near disaster, as Campbell points out: "At the San Francisco Conference six weeks later, the Argentine admission question, the regional issue, and the voting problem created such havoc as to practically destroy UN policy" (p. 129).

Most of the material for this study came from the extensive Stettinius collection, and by focusing on Stettinius an interesting insight is provided into his role in the formation of UN policy. Certainly Campbell's work is an important contribution to the history of the period.

JAMES J. DOUGHERTY
American Historical Review

RICHARD S. KIRKENDALL, editor. *The Truman Period as a Research Field: A Reappraisal, 1972*. [Columbia:] University of Missouri Press. 1974. Pp. 246. \$10.00.

This useful volume was designed to complement *The Truman Period as a Research Field* (1967), a work that grew out of a conference held in 1966 to appraise research and writing on the Truman administration. The new essays are particularly concerned with historiographical trends since 1966. Richard S. Kirkendall, who also edited the 1967 edition, has contributed an introduction that describes the development of the book and its relationship to the earlier publication. The book contains four substantial papers—two in the area of foreign affairs, by Robert H. Ferrell and Lloyd C. Gardner, and two in the field of domestic matters, by Harvard Sitkoff and Alonzo L. Hamby—and appraisals of the feature essays by five contributors to the 1967 volume: David S. McLellan, Barton J. Bernstein, Richard O. Davies, William C. Berman, and Elmer E. Cornwell, Jr. The result is a provocative set of conflicting interpretations of the Truman administration.

The writers fall rather neatly into two groups. Ferrell, Hamby, McLellan, and Davies are spokesmen for the more traditional, liberal interpretation; Gardner, Sitkoff, Bernstein, and Berman generally represent the revisionist, radical approach to the period. Three of the papers deserve special comment: Gardner argues the revisionist case with subtlety and restraint; Hamby offers a fresh and thoughtful review

from the liberal perspective; and Bernstein presents a spirited defense of the revisionist critique. The contributors agree that the historiography of the Truman period should now move on to the "second stage," to what Sitkoff describes as "analysis, rather than celebration and condemnation." They cite the need for a good overall study of the Truman presidency, for work that compares the United States with other parts of the world, for research into the social, economic, and intellectual developments of the era, and for more attention to local history and the scene outside Washington.

The present work, along with the edition of 1967, provides a valuable guide to the scholarly literature on the Truman period and the research that still needs to be done. It reveals the dynamic character, the increasing complexity, and the controversial nature of the scholarship in the field. It also suggests how the re-evaluation of the Truman administration helped spark, and fits into, the larger reinterpretation of American history that is now under way. The book owes a good deal to the Board of Directors of the Truman Library Institute, whose success may inspire the Eisenhower, Kennedy, and Johnson libraries similarly to encourage more systematic historical investigation of the 1950s and 1960s.

DEWEY W. GRANTHAM
Vanderbilt University

LISLE A. ROSE. *After Yalta*. New York: Charles Scribner's Sons. 1973. Pp. vi, 216. \$7.95.

ADAM B. ULAM. *The Rivals: America and Russia since World War II*. New York: Viking Press. 1971. Pp. vi, 405. \$10.95.

Although these two books deal primarily with Russo-American relations after World War II, the similarity ends there. Lisle Rose has written a footnoted monograph, which he calls an essay, that examines the origins of the cold war during the years 1945 and 1946. Adam Ulam offers an analysis of the interaction of Soviet and American foreign policy between 1945 and 1970—the treatment becomes quite sketchy for events after the Cuban missile crisis of 1962. Rose lists most of the relevant manuscripts and printed sources in his bibliography, but his book offers neither new facts nor new ideas, although it has the virtue of being pleasant to read. Ulam eschews citations (for which others have been roundly scored), but offers a trenchant, witty, and admittedly "realist" critique of American-Russian relations.

After Yalta suffers from narrowness of scope. Instead of writing with the purpose of objective historical analysis, Rose identifies the enemy in his preface. Taking careful aim at critics who claim that the United States "pursued an aggressive policy aimed at world . . . hegemony" and "cynically manipulated a . . . gullible national public," Rose reintroduces a variation of an old notion; the cold war came about—insofar as America could be blamed—because of confusion! This partial disinterment of the complaint that American policy was to have no policy is designed by Rose to reconcile American ideals with American actions. "Paralysis and drift, deferral and delay, became the rule" (p. 177). Except for a brief and unsatisfying glance at Russo-American relations during World War II, Rose makes no real systematic attempt to deal with the origins of the cold war. Rather he tries to explain and rationalize the policies of the Truman administration prior to 1947. Rose's facile lumping together of various leftist critiques of American policy sets up a misleading straw man. In fact, Rose is an eclectic who often accepts some of the key arguments of those he challenges. Perhaps a single quotation can best illustrate his curious position: "And so Americans set out to reshape their society and the world in the image of militant anti-Communism. . . . But they did so, . . . not in a spirit of aggressive self-confidence, but in fear and frequent confusion" (p. 183). Only the most polemical, radical critic would argue with that statement. Rose epitomizes a generation of historians who are torn between what they have been taught, what they would like to believe, and what more recent research has revealed. He is too honest simply to reject facts that do not fit his preconceptions, but in his struggle against simplistic conspiracy theses he offers the alternative of policy without conscious thought—an equally simplistic view. There are strong points, particularly his discussion of the general feeling of economic dislocation felt by Americans in 1945 and 1946 (including a very strong fear of depression—an argument usually presented by critics on the left), but the book appears largely as a summary of what we already know.

Not so with Adam Ulam's *The Rivals*. It is consistently provocative, interesting, logical, and ironically amusing, even if he is annoyingly preoccupied with student radicalism. Ulam knows exactly what he thinks, and he displays none of the uncertainty and ambiguousness that plague Rose. Ulam is arrogantly, but delightfully, "realistic."

To criticize his book is to reoffer many of the arguments that have previously been made against George Kennan, Henry Kissinger, Hans Morgenthau, and the like. Ulam invariably assumes the moral superiority of American goals, then pretends to ignore it. In the final analysis, realism without being tempered by ideals degenerates into mere power politics where might makes right. Hence Ulam writes of America's failure to take advantage of its virtual omnipotence in 1944-45, though he tempers that with a brief remark about not regretting that America had no desire to rule the world. He labels the actions of the CIA in Iran and Guatemala as "modest" successes, and he mourns the absence of a John Foster Dulles in the late 1950s when similar problems arose in Cuba.

More often, he pictures the United States as a naive, well-intentioned, often foolish giant that only occasionally used its power sensibly. Impatient and inflexible, American leaders failed to recognize Soviet compromises when they were offered. Ulam argues persuasively for example, that the United States missed a number of golden opportunities to lessen Russo-American tension, particularly regarding a neutralized Germany in 1947 and the Rapacki plan for a nuclear-free zone in Central Europe.

In addition to culpable ignorance about Russian policies and goals, Ulam finds another villain to blame for American failure to take advantage of its power: the "tendencies" of democracies. In spite of occasional disclaimers, the thrust of his arguments and his sarcasm is a condemnation of the moralism, impatience, and foolishness of both politicians and the public. The argument is an old yet superficially compelling one, for it appeals to that very sense of guilt that Ulam often ridicules as unrealistic. Unfortunately, that argument still has the same fatal flaw. It assumes that, freed from the restraints of a foolish and illogical public, the government will follow realistic policies. Not only can one question the "realism" of twentieth-century dictators, but to make such inferences without considering their implications for the society as a whole borders on irresponsibility. Certainly we have all had enough of foreign policy successes being offered as excuses for domestic authoritarianism.

WARREN F. KIMBALL
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THOMAS G. PATERSON. *Soviet-American Confrontation: Postwar Reconstruction and the Origins*

of the Cold War. Baltimore: Johns Hopkins University Press. 1973. Pp. xi, 287. \$12.00.

LISLE A. ROSE. *Dubious Victory: The United States and the End of World War II*. (The Coming of the American Age, 1945-1946.) [Kent, Ohio:] Kent State University Press. 1973. Pp. xiv, 392. \$10.00.

Thomas Paterson and Lisle Rose have added important contributions to the extensive literature on the cold war. Paterson, a representative of the revisionist school, and Rose, part of the recent response to revisionism, present many familiar and predictable arguments. Their books, however, offer more than reiteration of established themes. While the economic orientation of American policy has been emphasized in much revisionist writing, Paterson cogently focuses on the sources and ramifications of the American response to postwar economic problems. And while Rose defends Truman's approach toward the Russians during the first few months of his administration, he breaks with most orthodox accounts by sharply criticizing Truman's atomic policy.

Of the two books, Paterson's is the more tightly presented. He carefully develops the thesis that the post-World War II American quest for "peace and prosperity" encouraged an expansionist economic policy that proved significant in the erosion of Russian-American relations. Aware of the immense power they possessed, American leaders sought to utilize the need for American assistance in postwar reconstruction as a means of coercing the shattered countries of Europe to accept American economic and political objectives. In dealing with Russia and Eastern Europe, American policy proved to be shortsighted, insensitive, and counterproductive. A cautious economic policy without political strings would have rendered less likely the tightening of Russian control in Eastern Europe. In the cases of Finland and Yugoslavia, the United States acted wisely by not attempting to force those governments into the Western camp. In Czechoslovakia, on the other hand, American diplomacy from 1945 to 1948 failed to comprehend the Czech government's determination to remain neutral and to anticipate that economic coercion would weaken the position of the Czech moderates. The development of the containment doctrine resulted directly from American, not Russian, expansion. In the Iranian crisis, Russia sought limited objectives and showed a willingness to reduce tensions while the United States endeavored

to bring Iran, with its important oil resources, firmly into the Western sphere. This set the tone for the American response to the perceived Russian pressures on Greece and Turkey. The Truman Doctrine not only misinterpreted limited Soviet objectives, but provided the occasion for a formal and open commitment to the already well-established dual policy of containment/expansion. The Marshall Plan and the revitalization of western Germany represented the conclusive phases of America's effort to utilize reconstruction for its economic and political benefit.

Paterson's stress on the "peace and prosperity" objective results in minimizing other factors. Most important perhaps, he differs with the conclusions reached by John Lewis Gaddis in *The United States and the Origins of the Cold War* that public opinion and congressional pressures strongly influenced Truman's foreign policy. Observing that only one-fourth of the voting public in the 1940s was attentive to foreign policy questions and that Truman had little difficulty getting cold-war measures through Congress, Paterson finds little substantial questioning of Truman's policy. Yet as Gaddis demonstrated, the pressures to "get tough" were seen as real by the Truman administration and reduced the politically viable options available to Truman by 1946. One need not agree with Gaddis's observation that this meant the Soviet Union had the greater opportunity to avoid the cold war, but his study suggests the importance of examining whether the Truman administration believed that congressional and public opinion compelled a stronger anti-Soviet policy. Paterson's focus on "peace and prosperity" also tends to ignore the more general fears, aspirations, and perceptions of the American public. Also, the disagreements among policy makers are downplayed. Of course, no writer can possibly cover all that should be detailed and analyzed about the evolution of American cold-war diplomacy. On the whole, Paterson's book is an impressive revisionist statement. Written in a careful and lucid style, it reflects systematic research in published and unpublished sources; indeed, the rich notes and bibliographic essay attest a thorough synthesis of cold-war documents and literature.

Like Paterson, Lisle Rose contends that a cautious Russian diplomacy sought limited objectives in 1945, but he differs with revisionists in arguing that the United States pursued a similar policy. From the Yalta Conference to Hiroshima, the Soviets and Americans made

substantial progress toward resolving significant issues. Truman, concerned with the need for Russian assistance in the war against Japan, continued the accommodated course set by Roosevelt—especially by showing little interest in challenging Russian hegemony in Eastern Europe. Beginning with Yalta and continuing with the understandings reached at San Francisco and during the Hopkins mission to Moscow, Soviet and American leaders gravitated toward a settlement based on recognition of spheres of influence. Many officials questioned Truman's moderation, but Truman successfully resisted their pressures, at least until the ambitious and influential James Byrnes asserted firm leadership of the State Department.

Inept American atomic policy led, however, to a sudden deterioration of relations. Rose criticizes Truman for failing as a statesman; he should have acted "swiftly to give the Russians at least the promise of joining Britain and the United States in a joint nuclear club," for that "was the only way to stave off the sudden disaster in East-West relations" (p. 355). The bomb's "sudden use revealed a disturbing trait in the American character. The United States would seemingly throw self-restraint to the winds and employ unlimited power to achieve its primary objectives" (p. 366). Moreover with the war ending, Truman gave Byrnes, the atom-brandishing hard-liner, greater latitude in the direction of foreign policy.

In developing his thesis, Rose is generally convincing. By detailing the serious negotiation efforts and calling attention to the significance of the Pacific War in Russian-American relations, he places the developments of 1945 in a fuller context than is often the case, especially in the revisionist literature. Rose may understate problems that intruded on the prospects for accommodation; the legacy of the second front issue and Washington's procrastination on the Russian loan request, among other questions, foreshadowed serious problems long before Hiroshima. The reasons for Truman's reluctance to share atomic knowledge are not fully explored; on the basis of the recent scholarship of Martin J. Sherwin and Barton Bernstein, it would seem that Truman's decision must be considered against the background of four years of high-level planning on atomic energy policy. Having summarized the reasons that compelled the use of the atomic bomb, Rose's judgment on the "American character" seems unnecessarily harsh. One hopes that Rose's forthcoming volume on the emerging American cold-war diplomacy will deal more

fully with the sources and nature of American power.

Although writing from far different perspectives, Paterson and Rose find America utilizing its power, be it economic or military, unwisely and, in the process, bringing on the cold war. In the continuing debate on the origins of the cold war, their works will demand the serious attention of scholars.

GARY R. HESS

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VERNON H. JENSEN. *Strife on the Waterfront: The Port of New York since 1945*. Ithaca: Cornell University Press. 1974. Pp. 478. \$18.50.

From the days of John R. Commons, American labor history long remained the province of the labor economists. In recent years, historians have begun to explore and open up the field, but they have left one area—collective bargaining—almost wholly to the labor economists. The results of that monopoly, for better or worse, are evident in this book. No one could be better informed on the recent collective bargaining history at the Port of New York than Vernon Jensen, not only by virtue of his previous work on longshore hiring practices, but also because of his first-hand knowledge of his subject. This gives him, first of all, access to a large fund of confidential information. It also gives him a superb command of what is an uncommonly intricate subject, any of whose parts—the longshoring business, the disreputable International Longshoremen's Association (ILA), a host of public agencies, major technological changes—would challenge the scholar's skill. The hallmark of the best work on American labor relations—by Sumner Slichter and John T. Dunlop, for example—has been its grounding in direct observation and experience, and this book, while it does not indicate Jensen's connection to the New York waterfront, has the ring of the practitioner to it.

There is, unfortunately, a price to pay for this kind of expertness. For one thing, Jensen's questions do not go beyond asking why collective bargaining did not work better. Moreover, he focuses very narrowly on the negotiating process. Through all the twists and turns for over four hundred pages, the battle over each contract is recounted in stupefying detail. And all the developments are treated essentially as items on the bargaining table. It is true that, with the aid of an introductory "explicit frame of reference" and Jensen's observations as he goes along, the main themes can be discerned,

but not nearly so well as if the book had been organized to elicit and develop those themes. And certain issues cannot be answered at all, such as, why, after three decades of labor peace on the New York waterfront, an area of persistent strife should have begun in 1945, just the reverse of the experience on the West Coast.

It is probably because they have assumed collective bargaining to be a static phenomenon that labor historians have tended to lose interest as soon as the story reaches the point of union recognition. This book suggests the dynamic element in the process. Some of this doubtless reflects the peculiarities of New York longshoring: the abuse of the shape up, the impact of containerization, the internal politics of the ILA, the large public interest in labor peace. But the limits to collective bargaining that these facts reveal, especially as a mechanism for rational decision making and for holding the allegiance of the rank and file, can be perceived more generally on the current labor scene. One hopes that the discovery that conventional collective bargaining may not be the end of the line will lure labor historians into an area they have hitherto thought could only excite labor economists.

DAVID BRODY

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F. ROSS PETERSON. *Prophet without Honor: Glen H. Taylor and the Fight for American Liberalism*. [Lexington:] University Press of Kentucky. 1974. Pp. xi, 216. \$12.75.

Few progressives were left unseared by the post-World War II ordeal of refurbishing the social-reform aspirations of American liberalism and forging diplomatic policies toward the Soviet Union. While some progressives insisted that Roosevelt and Truman's enlarged welfare-state policies signified a malign departure in the American political tradition, others, such as Idaho's Democratic senator Glen H. Taylor, lamented the atrophy of domestic reform and the ascendancy of Soviet-American embitterment.

Since Taylor's critique of American liberalism anticipated certain aspects of cold-war revisionist scholarship, F. Ross Peterson is distressed that Taylor "has been almost totally ignored" and often "dismissed without even a footnote." This biography is animated by the conviction that Glen Taylor "was more than a colorful

ham actor, a hillbilly musician, and an egotistical opportunist." He was "a dedicated liberal humanitarian" who Peterson agrees accurately characterized his political career in a 1967 oral history interview: "A prophet is without honor in his own household."

This study is an extension of the author's 1968 dissertation at Washington State University. It is a "political biography" with no notable narrative or analytical qualities. It is competently researched and provides a straightforward, if often numbing and cliché-suffused, reportorial account of Glen Taylor from vaudeville actor and country singer to a single United States Senate term and then the vice-presidential nomination of Henry A. Wallace's Progressive party in 1948. Although Peterson claims that the absence of a single manuscript collection of Taylor's papers was a "major research handicap" and the reason "the biography is only a political biography," it is evident that the essential aspects of Taylor's political beliefs and attitudes are rather fully revealed in the available sources. The deficiencies of the book are more directly attributable to insufficient analysis and an unwillingness to provide more than an exposition of Taylor's views and activities as derived from the *Congressional Record* and newspaper accounts.

Students will find the book a convenient summary of Glen Taylor's struggle against what he considered the serious inadequacies of Harry Truman's supine domestic liberalism and recklessly provocative "anti-Communist" diplomacy. It does not, however, provide any interpretive analysis of post-World War II American liberalism or new perspectives on the Progressive party or domestic opposition to the cold war. If Peterson has rescued Taylor from oblivion, he has not avoided the difficulties of considering public figures *in vacuo*.

FRANK ANNUNZIATA
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Foreign Relations of the United States, 1948, volume 8, *The Far East: China*. Department of State Publication 8683. Washington: Government Printing Office. 1973. Pp. vi, 986. \$9.30.

This collection is the fourth volume to appear in the Foreign Relations series for 1948. Nine volumes are planned for 1948, although this information is not provided in the volume being reviewed. Indeed, lack of editorial guideposts such as this comprise the principal flaw in the book. Unlike some other volumes in

the series, it does not systematically review the sources utilized in compiling documents. (Sources outside the State Department are used, but how extensively they were combed is not indicated.) Nor does it reveal the criteria for deciding which documents to print here, elsewhere, or not at all. Thus, even though many readers will undoubtedly be curious about the paucity of political reporting on the internal affairs of China, they are not told that volume 7 for 1948 is also devoted to China or advised that it deals principally with political affairs.

Despite these rather minor lapses, the book contributes significantly to our knowledge of United States China policy during 1948. It makes this contribution by putting flesh on the skeleton traced in volume 7 for 1948 and in *United States Relations With China, With Special Reference to the Period 1944-1949* (Department of State Publication 3573, Far Eastern Series 30), popularly known as the *China White Paper*. For example, extensive coverage of the administrative, economic, legal, and technical factors that influenced the military assistance and economic aid programs of the United States complements the political and policy considerations treated more fully in the two works mentioned above. Since the volume makes its principal contribution by supplementing the data in these works, it seems most useful to conclude by briefly reviewing its contents.

The longest single section (306 pages) deals with United States military assistance to China. Next in size (243 pages) is United States economic aid to China. Much shorter sections cover financial relations between the United States and China (77 pages), the evacuation of Americans from China (137 pages), the status of United States naval and marine forces at Tsingtao (38 pages), and negotiations concerning the revision of the air transport agreement of December 20, 1946 (25 pages). Other items, although mentioned, receive minimal attention. These include efforts leading to the release of United States marines captured by the Chinese Communists, American opposition to an international police force for Shanghai during the transition period, and negotiations and representations on a variety of issues ranging from the final settlement of war accounts to censorship of American movies, joint exploration of minerals for atomic energy, and the evacuation of refugee groups from Shanghai through the International Refugee Organization.

ANDREW T. FORD
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ROBERT GRIFFITH and ATHAN THEOHARIS, editors. *The Specter: Original Essays on the Cold War and the Origins of McCarthyism*. New York: New Viewpoints. 1974. Pp. xiv, 368. Cloth \$10.00, paper \$4.95.

The twelve essays contained in this interesting and important book are products of young scholars laboring on the frontiers of a field in need of much careful and objective research. Many of the selections are based on recent doctoral dissertations. With the exceptions of two tiresome and predictable Marxist selections, an uninspired piece on Hollywood and the cold war and a weak and unconvincing interpretation of the American Civil Liberties Union, the essays are solidly constructed, well thought out, and attractively written.

Robert Griffith's introduction and his "American Politics and The Origins of 'McCarthyism'" are especially useful. He tersely summarizes and dismisses much of what has been written on the meaning of McCarthyism and concludes—as did Earl Latham several years ago—that the Second Red Scare was largely a political phenomenon rooted in the clash between liberal and conservative elites. McCarthyism, in short, had little directly to do with the vast majority of the American people. Griffith calls attention to the long history of anticommunism as a political device and contends that Joe McCarthy himself was of minor significance; he "was the product of anti-Communist politics, not its progenitor."

Donald F. Crosby, S.J. contributes a frank and stimulating discussion of the relationship between American Catholicism and our anti-Communist impulse. Among other things, he attempts to explain why Catholic elites—leading clergymen, editors, writers, politicians, and directors of fraternal organizations—played prominent roles in right-wing extremist activities. Pithy portraits of Francis Cardinal Spellman and Bishop Fulton J. Sheen may prompt further inquiries into the darker movements of these influential and controversial prelates.

Ronald Lora looks at several cold-war conservative intellectuals, such as James Burnham, and attempts to show why and how they actively promoted McCarthyism. Peter H. Irons ably describes the ultraconservative activities of the United States Chamber of Commerce during the late 1940s. David M. Oshinsky's "Labor's Cold War: The CIO and the Communists" depicts the powerful commitment to postwar anticommunism shared by America's top labor leaders.

The creation of the McCarran Act of 1950 is described in detail in an essay coauthored by William R. Tanner and Robert Griffith. (I find its antiliberal thrust objectionable, but the piece is rewarding nevertheless.) Richard M. Fried's superb study of the uses of the Communist issue in the state campaigns of 1950 reveals clearly how Joe McCarthy's reputation as a vote getter was exaggerated by the press and a number of nervous politicians.

Many readers will find Michael O'Brien's article on the little-known Cedric Parker case of 1949 the most original and fascinating contribution in the volume. Through meticulous research in Wisconsin, O'Brien gives us a view of Joe McCarthy at work in his home state that forever lays to rest the theory that the senator first stumbled upon anticommunism as a political weapon on January 7, 1950, following a dinner at the Colony restaurant in Washington, D.C.

On the whole, this is an excellent book, indispensable to the specialist and highly recommended to the general reader. Similar projects, it seems to me, should be encouraged. They would open opportunities for newer members of the profession and might well prove as valuable as the volume under review.

THOMAS C. REEVES
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EDWARD J. BACCIOCCO, JR. *The New Left in America: Reform to Revolution, 1956 to 1970*. (Hoover Institution Publications, 130.) Stanford: Hoover Institution Press, Stanford University. 1974. Pp. xvi, 300. \$8.95.

The activism of the 1960s has finally faded far enough into the past for us to examine it with minds relatively free of the distortions engendered by the passions of the conflict. We are not so far removed for these passions to have cooled off completely, but far enough for Edward J. Bacciocco, Jr. to write a most welcome history of the New Left in which he avoids the extremism of either left or right. His book is a lucid, evenly balanced treatment of the New Left from its origins in the late 1950s to its decline a decade later.

According to Bacciocco, there were fundamental differences between the movement of the early 1960s and that of the years after 1965. The Student Nonviolent Coordinating Committee, the "moving spirit" of the New Left in its early days, originally was devoted to the principle of participatory democracy. The aim of

the organization was to help the blacks to help themselves, and its first priority was the development of local leadership in the rural South. But progress was slow and the problems of the Mississippi Summer Project of 1964 contributed to a major change in policy. SNCC turned from community organizing, nonviolence, and cooperation with whites to the support of the concept of Black Power and soon began to lose its position as the most prominent organization of black radicals.

Bacciocco goes on to point out that the other major component of the New Left, Students for a Democratic Society, also changed significantly in the middle of the decade. SDS joined in support of participatory democracy and worked closely with SNCC in the early 1960s. Essentially reformist at the outset, SDS grew more radical and by 1965 had begun to shift to revolution and Marxism-Leninism. As it moved leftward, the organization became torn by factionalism until in 1969 it divided into three separate groups and faded into insignificance.

Bacciocco sees the increasing radicalization of the movement as the major reason for its failure to make a more profound impact on the country and for the virtual demise of the New Left in the late 1960s. SNCC was too impatient, he feels, considering the three hundred years of prejudice that had to be overcome. It had accomplished important things and "was probably premature in arguing that Black Power represented a necessary shift in strategy in order to achieve more for the American Negro" (p. 98). Similarly, the radical shift was fatal to SDS. "Instead of controlling its hostility to secure important political objectives," Bacciocco writes, "SDS chose to ignore the proven reform potential of the white middle class, accusing it of complacency and resistance to rapid change" (p. 234).

The latter statement reflects an approach to the subject that is the most troublesome feature of the book. The problem is at least partially rooted in a failure to define terms clearly and is apparent at the outset when Bacciocco refers to the New Left as "originally a collection of radical student reformers" (p. xiii). The words "radical" and "reformer" should not be used to describe the same person or group. There are radicals, whose aim is to overthrow a social, economic, or political system, and there are reformers, who seek merely to make changes in the existing system, but there are no radical reformers. The New Leftists were never reformers and the term should not be applied to them, even before 1965. It is more

accurate and less confusing to call them radicals throughout, while pointing out that they became more radical toward the end of the decade. If one accepts this, it then becomes somewhat difficult to go along with Bacciocco's criticism that the New Leftists did not act like reformers. He may very well be correct in his contention that they would have been more successful if they had followed the well-trodden path of reform, but the fact remains that they were radicals and therefore it should not be surprising that they acted like radicals.

But I do not want to end on a harsh note, for the merits of this book easily overshadow its flaws. If the historian's chief task is to bring order and meaning to the chaos that is history, Bacciocco has performed his task admirably.

HAROLD W. CURRIE

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CORRECTION: The publisher of Anatolii Andreievich Gromyko, *Through Russian Eyes: President Kennedy's 1036 Days*, reviewed in the April *AHR*, page 540, should have read Washington: International Library.

JAMES C. HARVEY. *Black Civil Rights during the Johnson Administration*. Jackson: University and College Press of Mississippi. 1973. Pp. xiv, 245. \$4.95.

The Civil Rights era is receding from us into that disordered and cluttered timespan of hazily remembered but not yet recorded history. Until critical historians arise to sift the evidence and tell us what we lived through, we are increasingly dependent upon the jumble of oral history, memoirs, and journalistic accounts floating in the churning wake of the movement. *Black Civil Rights during the Johnson Administration*, like its earlier companion, *Civil Rights during the Kennedy Administration*, both by James C. Harvey, fills a need by providing a well ordered and intelligent description of the policies of the executive branch of the federal government and the contents of federal legislation with regard to civil rights.

The book does not pretend to be more than it is, an initial attempt to collect and organize the flotsam of facts about the federal government's involvement in one of the two great and disruptive issues of national life in the 1960s. The book is not limited to description, but the analytical observations tend to be of the first order, the sort that are readily available to observant contemporaries of the events being

described. For instance, the slowing momentum of the civil rights movement is blamed on rising public concern over the Vietnam War and Lyndon Johnson's dependence on Southern congressmen to support his war measures. That is not incorrect, but it is incomplete. Little attempt is made to fathom the motivations or tactical choices of individual actors in the national drama, to explain the evolution of the movement in terms of its own internal dynamics, or to unravel the paradox of the onset of "disillusionment" within the movement around 1966, just at the time when enforcement of civil rights legislation was getting under way, real changes were beginning to show, and expressed confidence in the future among blacks generally was at an all-time high.

The author deals only superficially with the bureaucratic and electoral politics that swirled about the enactment and enforcement of civil rights legislation, but he does provide informative narratives of the major civil rights acts of 1964, 1965, and 1968. In addition, there are many very useful tables of data on employment, school desegregation, housing, and voting. The account is based upon published government documents and on extensive meanderings through the rapidly growing book and periodical literature on the subject. No attempt was made to interview participants or to use the available collections of transcribed interviews, presidential papers in the Johnson Library, or other primary source material.

The result is only a partial step forward, but a helpful one, refreshing in its calm admiration for Martin Luther King, Jr. and Lyndon B. Johnson.

SHELDON HACKNEY
Princeton University

CANADA

R. COLE HARRIS and JOHN WARKENTIN. *Canada before Confederation: A Study in Historical Geography*. (Historical Geography of North America Series.) New York: Oxford University Press. 1974. Pp. xiv, 338. Cloth \$9.95, paper \$5.95.

For the most part, this book makes for absorbing reading and presents an impression of the early development of Canada that departs refreshingly from some of the customary approaches to Canadian history. The authors' evocative sketch of the "lay of the land," their survey of settlers' perceptions of a newly adopted country, and their analysis of urban growth and the agricultural, commercial, and

industrial activities conducted in the various regions of Canada all demonstrate the contributions that historical geographers can make to an understanding of a nation's past. Throughout Harris and Warkentin have made a prodigious effort to call on the work of their colleagues and of such related scholars as demographers and historical sociologists. This is particularly true in the case of the sections on French Canada. In a number of instances, however, they appear to neglect the work of historians who could have helped to inform their chapter on Ontario—notably T. W. Acheson, F. H. Armstrong, Leo Johnson, Douglas McCalla, and H. V. Nelles, who have written on such themes as trade, settlement, land policy, and the emergence of the Canadian business community.

A more basic shortcoming of the book is the built-in anomaly, if not curiosity, boldly spelled out in its title and constantly confronted in its text. At a time when some scholars are inclined to minimize Confederation, essentially a political and constitutional event, as a landmark in the social, economic, and cultural growth of Canada, the authors accord it that kind of compartmentalizing significance that would have been perfectly acceptable to the historian and political scientist of a generation or two ago. This form of structuring seems out of keeping with the otherwise innovative approach that they adopt as historical geographers. Clearly a good many developments—for example, urbanization, at least of sorts—originated before Confederation was even thought of, a point amply illustrated in the book, and continued to flourish without any apparent indebtedness to what Sir John A. Macdonald and his associates did in the 1860s.

Among the reasons offered for selecting Confederation as a cut-off point are that neither author had made an intensive study of late nineteenth-century Canada and that virtually no geographical literature had been prepared on that period. Yet this latter deficiency is in places noticeable for the period that they examined. This, however, does not deter them from writing it up, with the result that some parts of the book, particularly the treatment of the Northwest, could have been read in any standard historical account. (And, again, there is a bibliographical shortcoming: no reference is made to the work done on the subject of western expansion by J. S. Galbraith and A. C. Gluek.) In any case, resources and research aside, one is left wondering if another terminal point would have been more appropriate for a

work such as this, say, the turn of the century with its "wheat boom," massive European immigration, and large-scale urbanization, factors that governed a wholesale reshaping of the Canadian social and economic landscape.

Although the bulk of the book is well written, the same cannot be said for the way in which some of its chapters are organized. The chopping up of these into numerous sections occasionally produces an irritating repetition and frequently erodes the reader's interest in an otherwise imaginative presentation. In the chapter on the Western Interior, for instance, we are twice told, with a little different wording, about the results of expeditions dispatched to evaluate the resources of the prairies.

Overall, however, and in spite of the flaws cited, this study constitutes a good start. As the authors observe, they set out to write a "deliberately provocative" work in the expectation that others would respond and after further research add to this introductory account or revise its findings. It is to be hoped that they will not be disappointed.

C. M. JOHNSTON
McMaster University

DESMOND MORTON, *The Canadian General: Sir William Otter*. (Canadian War Museum, Historical Publication number 9.) Toronto: Hakkert, 1974. Pp. xix, 423. \$12.95.

Sir William Otter's career, from the day he was accepted as a private in the Second Battalion, Volunteer Militia Rifles, the Queen's Own, in 1860, until his death as a full general in 1928, is a summary of nearly seventy years of Canada's military history. The invasion of Upper Canada by Irish-American Fenians, the confrontation with strikers on the Grand Trunk Railway, the battle with Poundmaker's Cree Indians at Cutknife Hill, the preparations for war with the United States over the Venezuela boundary, the marches and charges against the Boers in South Africa, the celebration of the Quebec tercentenary, the internment of enemy aliens during the Great War of 1914-18, the reorganization of the Canadian Militia after the First World War: Otter took part in each of them. Always he sought to impose British traditions of discipline upon the Canadians with whom he served or whom he commanded. He was uncompromisingly professional in what was essentially an amateur army at all times and in all circumstances.

The Otter story is set against the background of the political and social life of post-Confed-

eration Canada. It brings out the strengths and weaknesses of a volunteer force, the antagonisms between the militiaman and the regular, the national rivalries between the Canadian and the Englishman, and the impact of party politics both within and upon the Canadian military organization. No less relevant and interesting, however, is the account of Otter struggling to restore the family name and fortune. In an American setting Otter might well have been a Horatio Alger figure; because the setting is Canadian, he was, instead, a Henty hero. His good name was always there. It merely needed a bit of refurbishing. And, in the end, a knighthood and the perquisites of rank did the trick. It is all very Victorian; but, at long last, life in Victorian Canada is beginning to excite the interest of Canadian historians.

The Canadian General is a good book. It is written, and well written, by one of Canada's most promising young military historians, a graduate of the Royal Military College and a Rhodes scholar, who gave up a military career for an academic one. Although the author is the great-grandson of Sir William Otter, his book is not a work of hagiography. It is a fair, balanced, and accurate picture, warts and all, of the most important military figure in Canada's first half century.

GEORGE F. G. STANLEY
Mount Allison University

JACQUES ROUILLARD, *Les travailleurs du coton au Québec, 1900-1915*. (Histoire des travailleurs québécois.) Montreal: Presses de l'Université du Québec, 1974. Pp. xii, 152. \$4.00.

This brief monograph on the cotton textile industry in Quebec from 1900 to 1915 reflects a new interest of French-Canadian historians in labor history. The Quebec textile industry has much to offer the labor historian, for its workers were notoriously underpaid, badly mistreated, and consequently prone to strike. Inevitably, much attention is devoted in this study to Dominion Textile, the combine formed in 1905 that gradually monopolized an industry in which twenty-four different companies had participated in 1885. But in much of the period considered here, there was still competition.

An introductory chapter traces the historical background of the industry in Quebec, including the tariff protection that, coupled with cheap labor, made it viable. A second chapter is devoted to the textile worker and his unhappy lot, while a third deals with the unionizing efforts of the Knights of Labor and the

Federation of Textile Workers of Canada. A final chapter is devoted to the major strikes of the period at the Magog, Montmorency, Hochelaga, and Saint Henri mills. In conclusion the author suggests that the availability or scarcity of labor was the basic factor governing salaries and the employers' attitudes toward unions. It is not surprising that William Lyon Mackenzie King, as chairman of the 1908-09 royal commission on management-labor differences in the industry, adopted management's views.

MASON WADE

Cornish, New Hampshire

JOHN A. MUNRO and ALEX. I. INGLIS, editors. *Mike: The Memoirs of the Right Honourable Lester B. Pearson*. Volume 2, 1948-1957. [New York:] Quadrangle/The New York Times Book Company. 1973. Pp. xi, 344. \$12.50.

The structure of the three volumes of the Pearson memoirs is based upon the three stages of his career. Volume 1 (*AHR*, 78 [1973]: 1148) dealt with the quiet, able civil servant of the Department of External Affairs in Ottawa. Volume 2 deals with Pearson the politician, the secretary of state for External Affairs from September 1948 to December 1956, from the formation of NATO to the Suez crisis. Volume 3, when it comes, will be devoted to his role as a political leader, and later prime minister (1963-68), until his death in 1972.

The Pearson of the first two volumes, and almost certainly of volume 3, does not change much. He seems always the same agreeable, one might almost say guileless, son of an Ontario Methodist parsonage. His answer when a reporter phoned to tell him he had been awarded the Nobel Peace Prize for 1957 was, "Gosh!" It is hard not to like Pearson: it is not always easy to admire him. This book illustrates him at every turn, his humor, his charm, his sometimes stodgy verbosity, his naiveté. He seemed to expect great men to be great and villains to be villainous. For example, he was surprised, even shocked, at Nehru's remark about Gandhi. "You know," Nehru had said, "he [Gandhi] really was an awful old hypocrite." Pearson's unspoken reaction seemed to be, "Gandhi, a hypocrite? Impossible!" Equally, Pearson found himself next to Vishinsky at dinner after the Russian had spent the whole afternoon delivering a vicious polemic against Western diplomacy and all its works. Vishinsky was perfectly charming. "Personally," said Pearson a little puzzled, "I prefer my 'villains' to be more easily recognized when off duty."

The main themes in this volume are the formation of NATO, the Korean War, and Pearson's role in the Suez crisis in helping to persuade the Americans to support—and Britain and France to accept—a UN peace and a UN peace force. Pearson's action was broadly supported in Canada, though he endured some bitter Conservative criticism in Parliament over stabbing Britain in the back and running chores for the Americans. He did neither. He was fair with Eden, and he once told John Foster Dulles that he too often used a niblick when he ought to have been using a putter.

There is much value in this book even to those who are unfamiliar with Canada. It gives a sense of perspective, a view from Ottawa about both the British and the Americans, and a touch of northern coolheadedness, one of Pearson's pre-eminent virtues. He reversed the familiar motto, "Don't just stand there, do something." His advice was, "Don't do anything, just stand there—and think."

P. B. WAITE

Dalhousie University

LATIN AMERICA

EDWARD J. GOODMAN. *The Explorers of South America*. New York: Macmillan Company. 1972. Pp. viii, 408. \$15.00.

This book presents a full record of how the South American continent was circumnavigated and traversed from points around the compass to reveal its outline and features. It tells which point was reached by whom first, and it traces the route there and back. I should think that such a survey inspired the book, rather than J. B. Brebner's *Explorers of North America, 1492-1806* (1971), which the author acknowledges. Mr. Goodman was surprised to find that there was no companion volume for the southern continent, and there still is not, nor could there be one. The author gives us a very different book.

It is an account arranged in chronological order that identifies the leading motives for exploration animating the explorers since 1492. "The First Century: Discovery and Conquest" gives way to "The Age of Expansion: For God, Slaves, and Gold." Two more parts, "The Early Scientific Explorers" and "The Great Age of Scientific Exploration," lead us up to Colonel Fawcett. We are also told how the results of each expedition reached the public. This is the sum of the content, and a rich harvest it is indeed.

Mr. Goodman has brought together in one volume a more ample list of explorers, their companions and aides, and descriptions of their exploits than has hitherto been available in English. The book is in fact a richly annotated catalog since historical dimension is lacking, except for set pieces of text that attempt to relate the titles of the chapters to the narrative of the explorers' voyages. This is history by assertion only. The book is no geography either. The continent revealed is not of first concern. Five maps found scattered through the book are not listed or credited, and they barely suffice to identify points on the map. The pictures of men and landscapes bound in the center are closer to the text. The author conveys a lot of information and awakens curiosity. His sympathy seems most engaged when he can check accounts of travel and scientific data by his own experience of travel over wide regions of the continent. The historian of exploration is given ample clues to the author's bibliographic orientation in notes appended to the chapters and bibliography. Disagreement on facts arising from this source leave the reader to do his own exploring, and he can do so now from a solid base camp.

URSULA LAMB
University of Arizona

MARY ELIZABETH THOMAS. *Jamaica and Voluntary Laborers from Africa, 1840-1865*. (A Florida State University Book.) Gainesville: University Presses of Florida. 1974. Pp. v, 211. \$12.00.

In no place was the decision of Parliament to abolish slavery throughout the British Empire in 1834 to have more severe repercussions than in Jamaica. The transition period of "apprenticeship" proved unsatisfactory. Rather than constituting a free labor pool for the plantation-dominated economy, former slaves chose a smallholder existence in the hills, engaged spasmodically in remunerated employment, or drifted into vagrancy. By 1840 Jamaica faced a labor crisis. Attempts by successive British governments, the Colonial Office, the island legislature, and governors, commissions, and individuals to find solutions to this problem in the following quarter of a century are the theme of this monograph.

Largely unsuccessful efforts to drum up migration from North America and Europe only served to convince imperial and colonial authorities that the solution lay in West Africa in general, and in Sierra Leone in particular.

Using government documents and publications from Jamaica and Great Britain, Professor Thomas has unraveled with considerable skill and great clarity the changing policies and strategies ranging from voluntary migration to "assisted migration" and imperial supervision. She illustrates well how failure was born of factors over which Jamaica herself had no control: political pressures and economic and ideological considerations in England, to say nothing of changing governments in London, and sociological and cultural factors in Sierra Leone. This is not to exculpate the island legislature, whose indecision, vested self-interest, or simply poor judgment provoked dissent rather than solution. Successes there were, but of a temporary nature. Windfalls took the form of captured slave cargoes, Indian labor proved a palliative, but neither the colonial government nor the British Parliament found an overall solution. Economic stagnation, social unrest, mutual distrust, and frustration were the price paid for such experiments.

This is a potentially dramatic history possessed of an almost "whodunit" level of suspense. Thomas's easy style and concise description chart the reader through the murky waters of conflicting policies and the divergence between imperial directives and colonial realities. Having gained the reader's confidence by her scholarship, she disappoints by limiting herself to a mere presentation of evidence. The reader's early interest dies in a welter of correspondence from one authority to another, devoid of interpretation or analysis. That objectivity is a praiseworthy quality in a historian is undeniable, but it should not become a shield behind which the historian can seek cover from the responsibility of passing judgment. The reader seeks in vain for a personal assessment, evaluation, or analysis of the respective merits of a policy and is left with the unfulfilled hope that the author would emerge from behind her sources and provide a considered overview. Qualifiers such as "reportedly," "reputedly," or "presumably" are no substitute for a decisive yea or nay. Many of the issues described—the nature of the colonial pact, insensitivity within the different organs of government to overall objectives, colonial distrust and imperial dominance, problems of a plantation economy in a postemancipation era—were not only common to British possessions in the New World, but also to the colonial empires of France, Spain, Holland, and Portugal. Such comparisons are absent; comparisons to Trinidad and Demerara are limited to asides, and the contrasting situa-

tion in British Guiana (so ably described by Alan H. Adamson in *Sugar without Slaves* [1972]) is ignored. Many questions are suggested but remain unanswered. How well did the Africans assimilate? Did the Sierra Leoneans preserve any sense of identity in a New World environment? Was there conflict stimulation with Creoles or Asiatic migrants? The cultural and social implications are largely ignored. Perhaps in all fairness it must be admitted that such themes lie beyond the immediate scope of Thomas's prism, but the book would have been enhanced by brief sallies into these areas. However, it is here that this otherwise commendable study is found wanting, namely, in the reduction of an essentially human and vital phenomenon of migration and survival to an exercise in administrative history.

A. J. R. RUSSELL-WOOD
Johns Hopkins University

PETER L. EISENBERG. *The Sugar Industry in Pernambuco: Modernization without Change, 1840-1910*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xvi, 289. \$15.00.

This is an exemplary case study in plantation economy in a period generally lacking in significant historiography. The plantations of Pernambuco produced about a third of Brazilian sugar exports and supported one of the most powerful groups in Brazil's political structure. The nineteenth century was for that province a time of unrelieved crisis because of beet sugar competition, the closing of first the British and then the United States markets, and finally the abolition of slavery. The planters were obliged nevertheless to reinvest in new techniques, mainly railroads and steam-powered grinding mills, in order to salvage a part of their trade. On the whole the response of the planters was inadequate in the sense that southern Brazil far outdistanced them in development, and Pernambuco became a depressed region, in every manner an internal colony of the nation. On the other hand, changes were accomplished quickly enough to preserve control of the land by the latifundists, and even abolition caused no real relaxation of their grip on local society. The use of state subsidies to effect the necessary transition in sugar production might be used as a model of bourgeois expropriation of state resources, and the social consequences are starkly exposed, since the beneficiaries were slave drivers as well as land monopolists. Much material is pre-

sented that is of use in tracing the outlines of the economy, since the author explains financial institutions, commercial intermediaries, government incentives and taxes, and methods of processing. Sugar-cane growing is not considered as a natural process, however, and there is very little here on soil, climate, and ecological relationships.

The author makes valuable comparisons between Pernambuco's plantations and those of other regions, principally Cuba. He draws on an exhaustive knowledge of certain aspects of the sugar industry, and his discussion of other theoretical studies of the plantation system is very useful. In several regards his analysis contributes to significant debates, in particular the question of the degree to which the plantation elite was in control, which reaches in Brazilian historiography an extremity of reducing the masses to mere objects, whose own acts were irrelevant to deciding social change. This is a bleak picture, and one questions it because it is especially useful to the elite in perpetuating their dominance. In this study it is claimed that abolition was brought about by the slave owners themselves simply because slavery had become unprofitable. This thesis collides with other recent writings on Brazilian slavery. It may be true that the planters of this region were apathetic concerning the prospects of slavery, but the wage regime does not appear to have been any less expensive, according to the author's calculations. It is possible that the conclusion arises out of the quite exclusive concern of this study with the landowners' outlook, especially their business preoccupations. The workers' actions in this context are minimized because labor, as an economic factor, was in abundant supply. Yet so was it also in Haiti. Nevertheless, Eisenberg's arguments cannot be ignored, and his careful and intelligent monograph is essential to any student of Brazilian economic history.

WARREN DEAN
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RODOLFO F. ACUÑA. *Sonoran Strongman: Ignacio Pesqueira and His Times*. Tucson: University of Arizona Press. 1974. Pp. x, 179. Cloth \$10.50, paper \$4.50.

Ignacio Pesqueira belongs to a small group of *caudillos* who rose to power in northern Mexico during the decades after 1850, at a time when civil war and French intervention greatly reduced the power of the central government and the last waves of American annexationism

threatened the whole border area. The history of developments in the eastern half of this area—the middle and lower Rio Grande Valley—are much better known than those in Chihuahua and Sonora, so that Acuña's concise, scholarly monograph makes a valuable contribution to both United States and Mexican historiography.

Pesqueira managed to fend off American filibusters and resist secessionist temptations, but otherwise he did little of positive, long-range value for his native state. In goals, methods, and interests he was much closer to Antonio López de Santa Anna than to the later Porfirio Díaz. Pesqueira spent most of his active career alternately feuding with rivals and relaxing at his hacienda; and when American capital began to offer new means of development near the end of his life, Pesqueira was too old and habit hardened to make much use of them.

Limited by the destruction in 1915 of Pesqueira's family papers, Acuña has wisely written a life-and-times biography in which the almost insoluble border problems often bulk larger than the personality and exploits of his hero. Acuña's research is thorough and sound, his style a bit stiff but quite serviceable, and he has preserved an admirably objective attitude toward Pesqueira and toward American aggressiveness, despite his own Mexican background. It is a pity that he has found it necessary to refer to citizens of the United States as "North Americans," an unidiomatic term that should not have been acceptable to an American scholarly press.

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THEODORE E. NICHOLS. *Tres puertos de Colombia: Estudio sobre el desarrollo de Cartagena, Santa Marta y Barranquilla*. Bogotá: Biblioteca Banco Popular. 1973. Pp. 299.

This study of the impact of the transportation revolution on Colombia's northern coast is a well-done narrative of the attempts of a colonial society dislocated by its war for independence and the slow, costly, and sometimes futile efforts to rebuild an economic life with little more than the will of a handful of people. The process culminated in the 1900s in a daring effort to make navigable the mouth of a lazy, weak river, and it caused Barranquilla to emerge as the leading port of Caribbean Colombia.

The author traces the origins and travails of the projects with which visionaries tried to link

the Caribbean ports with Bogotá. Most of the projects were doomed: it was a matter of economics rather than a lack of determination, as the author indicates (p. 288). The vastness and emptiness of the Magdalena River Valley rendered every project a money loser from the start. Also, the planners' attempt to connect railroads to a natural but extremely undependable waterway caused considerable and costly damage to any project undertaken.

This study is interesting and well researched, and the Spanish version has been enhanced by the ability of the translators. Nichols has covered over 150 years of projects, many of them failures, that remain poorly documented. The organization of the several economic themes inherent to a study of this type is excellent, and the ability of the writer to keep threads together deserves praise.

There are, however, some points of disagreement. Little attention is paid to the human element. With the exception of references to President Núñez, there is no mention of the politicking involved in the planning of the projects. Overall economic conditions and the demographic distribution of the country also were not fully exploited in explaining the failures. Nichols glosses over Barranquilla's civic spirit that helped to build a city out of a "dusty, sandy hamlet," plan the remarkable Bocas project, and contribute to the founding of banks and even of an airline, the second oldest in the world. Finally, although Dr. Nichols takes into account while dealing with Cartagena (p. 271) that discrepancies in population figures are also due to changes in political subdivisions, he ignores it completely while dealing with Santa Marta (pp. 156, 162, 175).

This work, which was originally a doctoral dissertation, was published under the suggestion of Professor James F. King and with the interest of the Colombian historian, Rodolfo Segovia. Both should be highly commended for bringing to light, and in Spanish, a work that deserves notice in Colombia and the rest of Latin America.

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College at New Paltz

RONALD H. CHILCOTE. *The Brazilian Communist Party: Conflict and Integration 1922-1972*. New York: Oxford University Press. 1974. Pp. xv, 361. \$15.00.

JOHN W. F. DULLES. *Anarchists and Communists in Brazil, 1900-1935*. Austin: University of Texas Press. 1973. Pp. xvii, 603. \$12.50.

The last decade has witnessed a growing volume of scholarly literature dealing with the long-neglected subjects of the labor movement and leftist organizations in Latin America. While Brazil has benefited from this increasing attention, only now have two studies appeared that focus on the Communists and anarchists, the major radical groups in the history of the Brazilian labor movement. Ronald H. Chilcote offers us a critical analysis of the Brazilian Communist party to 1972, while John W. F. Dulles presents a wealth of factual material on the anarchists and Communists through 1935. Although narratives like Dulles's are no longer fashionable among historians, who often find themselves indebted to studies on recent history by political scientists like Chilcote, Dulles has uncovered an abundance of information useful for interpretive history.

Both authors concentrate on the later part of the time periods their books span. Professor Dulles covers the years up to 1917 in forty pages, less than one-tenth that devoted to events from 1917 to 1935. If he had utilized his preferred and extremely valuable source, newspapers, for the earlier period, his book would be even more weighty. Not only could he have found several anarchist groups and newspapers in Brazil before 1900, but also information on socialist-anarchist rivalries and feuds useful for comparisons with the later anarchist-Communist conflicts to which he devotes considerable attention, as he does the activities of the Communist party's intellectual leaders in the 1920s. Dulles's focus is on leaders, although no one really led the unorganized Brazilian working class during this period. Professor Chilcote, relying primarily upon material available in book form, concentrates on the post-1945 period. He tends to oversimplify some developments during the Communist party's initial years and to show little interest in the party's role in the Brazilian labor movement. Chilcote's major contribution lies in the second half of his book. Treating the Brazilian Communist party as a miniature political system and as a social group, he analyzes party structure, leadership, and membership, as well as party relations with local competitors, and evaluates the party's policies and its dealings with ruling and nonruling

Communist parties. Chilcote views the party as having "evolved into an organization controlled by a few leaders whose power was highly centralized and anti-democratic and whose policies became internally conservative"; it became "merely a force of limited pressure and moderation within the established order" (p. 211).

While Dulles's book lacks the conceptual framework and formulations of Chilcote's, it is rich in new information drawn from newspapers and personal interviews. With the partial exception of the regional study by Azis Simão, *Sindicato e estado: Suas relações na formação do proletariado de São Paulo* (1966), previous books dealing with the early Brazilian working class and labor movement were not based on research in labor newspapers. Although it is politically inexpedient to attempt to consult unpublished material still in police hands, as Chilcote notes, and many pertinent documents have been lost over the years due largely to widespread government repression, both books would have benefited from an examination of the scattered, incomplete documentation on the earlier periods, including some police records, available in public archives.

Dulles has not considered the implications of much of his research, and major trends and developments get lost among the details and quotations. While keeping track of the large number of labor and party figures is not an easy task for the reader of either book, Dulles captures many individual personalities who remain little more than names in Chilcote's study. Unlike Chilcote's, Dulles's book contains many excellent photographs and newspaper cartoons, as well as an appendix listing the prices, though unweighted, of basic commodities for São Paulo from 1917 to 1935. Chilcote's annotated bibliography and list of Communist and leftist periodicals should prove most useful; his informative and extensive, though inconveniently placed, footnotes provide an extensive "survey of the literature." Both studies, complementing one another, help fill long-standing needs and prove to be useful contributions.

JUNE E. HAHNER
State University of New York,
Albany

Communications

A communication will be considered only if it relates to an article or review published in this journal: publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication, whether concerning articles, review articles, or reviews, be no longer than 300 words. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

Thomas C. Cochran ("The Business Revolution," *AHR*, 79 [1974]: 1449-66) poses an opposition between his explanation of the Industrial Revolution and the "classic one" that "assigns a primary and basic function to technology" (p. 1449), citing as examples of this classic approach David Landes, Phyllis Deane, and Peter Mathias. I would argue that of these, only Landes makes technology central to the Industrial Revolution, and that the two others are in agreement with economic historians in general in not doing so. It is true that economic historians have usually given due recognition to the importance of technology, and many of them, like Mathias and Deane, devote whole chapters to the inventions associated with the Industrial Revolution. However, contrary to Cochran's assertion, this recognition does not necessarily reach to their assigning to technology a "primary and basic" role, if by this we mean a causal role in the same sense as that ascribed by Cochran to his "Business Revolution."

Arnold Toynbee, for example, while giving due weight to the accomplishments of the in-

ventors and engineers of the Industrial Revolution, still insisted that "the essence of the Industrial Revolution is the substitution of competition for the medieval regulations which had previously controlled the production and distribution of wealth" (*Lectures on the Industrial Revolution* [1928], 64). Paul Mantoux stated that "the use of machinery itself, important as are its consequences is only a secondary phenomenon" (*The Industrial Revolution in the Eighteenth Century* [1961], 41). H. L. Beales emphasized as did Mantoux the primacy of the social and economic environment and, for example, while acknowledging the importance of Watt as an inventor, called him "the child of his age" (*The Industrial Revolution 1750-1850* [1967], 55), which can only imply a dependent role. Maurice Dobb, writing in the Marxist tradition, considers that division of labor "prepared the ground for machine production" (*Studies in the Development of Capitalism* [1963], 145) and makes the Industrial Revolution an inevitable development of capitalism.

As to the authors particularly cited by Cochran, Mathias actually argued that the term "should not be used to denote industrial or mechanical innovation" (*The First Industrial Nation* [1969], 1), since innovations occurred before the eighteenth-century Industrial Revolution, if not as frequently, and many industries remained unchanged until well into the nineteenth century. In fact, he argued that "the concept implies a fundamental change in the structure of an economy" (*The First Industrial Nation*, 2). As to Deane, also mentioned by Cochran in this connection, it is true that she lists technical innovation as one of the elements constituting the Industrial Revolution (*The First Industrial Revolution* [1965], 1) and devotes an entire chapter to invention. She does not, however, give invention a more or less primary role than the other factors she mentions, which are social and economic.

With different emphases, the economic historians cited, and more recent writers like John Hicks in his *Theory of Economic History* (1969) and Douglass C. North with Robert P. Thomas in their *Rise of the Western World: A New Economic History* (1973) have looked to various economic or social factors or combinations of these rather than to technology to explain the Industrial Revolution. Cochran's "Business Revolution" does not offer a new approach but rather a continuation or variation on this tradition.

The recent tendency, exemplified in Landes's work, to see the central meaning of the Industrial Revolution in its technology derives in part from the researches in the last decade in the history of technology and culture that have deepened our understanding of the rise of Western technology and science. I suggest that a theory of the Industrial Revolution that excludes this background diminishes rather than improves our understanding of that unique event.

JOSEPH BURLOCK
Poquoson, Virginia

PROFESSOR COCHRAN REPLIES:

I differ very little from the views expressed by Mr. Burlock, except that I meant to show that while Deane and Mathias quite specifically recognize the importance of business and social structure, the sequence of events is less distinct than in the United States, and hence did not lead them to a sharp formulation. The thesis that the development of specialized, interconnected business activities is more a necessary cause or essence of the "revolution" than is the invention of new technology can be illustrated far more clearly from a brief period in American history than from that of England or the Continent.

THOMAS C. COCHRAN
University of Pennsylvania

TO THE EDITOR:

I write this tardy letter to express my dismay at reading A. O. Sarkissian's review of Halil Inalcik's *The Ottoman Empire: The Classical Age, 1300-1600* (*AHR*, 79 [1974]: 205-06). Unfortunately for Inalcik and the readers of the *AHR*, Sarkissian appears to have used the review as a personal forum to express contempt for Ottoman society and culture rather than to present a reasoned and evenhanded evaluation of the

scholarly merits or defects of the book. Inalcik's study is a careful overview of a society that at its peak was acknowledged both by its contemporaries and subsequent scholars as a vital and significant part of Western history. Considering the scope of this book, Sarkissian's examples of strangled rivals and decapitated advisers as "material already known to serious students of the subject" are pretty, for they pass over without comment the excellent synthesis of Western- and Turkish-language materials on such topics as international trade, provincial organization, urbanism, and popular culture.

Worst of all is Sarkissian's scornful condemnation, "The claim that the Ottoman Turks have given us a 'civilization' is hardly admissible, for where fanaticism was unrestrained, the whirling and wandering dervishes and other mystic orders were given a relatively free rein, and where administrative and combative forces were mostly of slave origin, 'civilization' as understood by all civilized people cannot flourish." What is such a statement supposed to mean in relation to Inalcik's book? It is certainly not accurate for the period covered by the book—a time when refugees from the Christian world fled to the toleration of Ottoman society. It is obviously a reflection of personal bias, not of scholarly judgment.

Considering the number of well-known Ottomanists in the United States alone who could have reviewed Inalcik's study (some of whom reviewed it for other scholarly journals), I question the wisdom of soliciting a review from someone so vehemently anti-Ottoman. But more important, for the *AHR* subsequently to print such a review is doubly damaging, since those unfamiliar with Ottoman studies may accept it at face value, while the more knowledgeable immediately recognize its superficiality and unfairness.

ADDEANE S. KELLY
University of Texas, Austin

MR. SARKISSIAN REPLIES:

I shall not use the full quota of words allowed me, since I am not disposed to dignify Professor Kelly's scurrilous criticism and brazen fulminations against me, except to say that I began my review of Halil Inalcik's *The Ottoman Empire: The Classical Age, 1300-1600* by praising it as a "valuable contribution," and I concluded with additional praise for its "fifty-five pages of rich scholarly trimmings," its genealogical chart, its chronology, its helpful glossary of Turkish terms, its detailed chapter notes,

and its select bibliography. My only criticism was that the Ottoman Turks could not be credited with creating a civilization. As warriors and organizers for wars the Ottoman Turks were superb, but in nearly all other forms of human activity they were unprepared, inexperienced, and ill equipped, not to say incapable of producing a civilization.

I suggest that Professor Kelly read my review carefully before raging against me violently and before questioning the editor's wisdom for publishing it.

A. O. SARKISSIAN
Library of Congress

The following letter is in reply to a communication from Gordon E. Geddes (AHR, 80 [1975]: 205-06) concerning the article, "Death and Dying in Puritan New England," by David E. Stannard (AHR, 78 [1973]: 1305-30).

TO THE EDITOR:

Because of the *AHR*'s recent and stringent requirements concerning space, I have considerably less room to answer Mr. Geddes's criticism than he had to make his charges. This is unfortunate since much of his criticism involved both apparent misunderstanding of language and outright misrepresentation of my ideas. But I shall try—though I am forced to limit myself to what I see as his three major points of criticism.

First, Geddes claims that I see the Christian view of death as "static." I do not. It is true that my treatment of traditional Christian attitudes is more diagrammatic than it would have been had it not been limited to two pages of background for the main themes my article develops, but that it is not static becomes evident when Geddes attempts to document his charge. He claims, for example, that I view death as having been "easy" for pre-Reformation Christians, and he implies that I regard the *Ars Moriendi* as support for such a contention. But on the contrary, I specifically point to the *Ars Moriendi* as one piece of evidence that in the Christian world of the fourteenth and fifteenth centuries fear of death was in fact a dominant characteristic of religious art and literature. What I do say is that Christianity provided a belief system that made fear of death at least understandable and intellectually tolerable. The complexity of this point, as developed at greater length in my article, seems to have eluded Geddes. As a result, he is criticizing me

not only for something I did not say but for something I took some pains explicitly to reject.

Second, Geddes states that "Stannard views death as either reward or punishment while his sources, medieval and Puritan, treat it as both reward and punishment." Now, how I view death is irrelevant. But what I have to say about the Puritan view of death is that it was on this point ambivalent, that it was, in my words, "both punishment and reward." Again, Geddes is criticizing me for something I did not say by repeating precisely what I said.

Third, the most serious and irresponsible charge that Geddes makes is that I have mishandled sources in developing my thesis of "consistant [*sic*] patterns" (a phrase that, despite his quotation marks, does not appear in my article) in the Puritans' approach to death and dying. His support for this charge is based on two points—that Mather's *Thoughts of a Dying Man* (1697) was "not a compendium of saintly deaths"; and that I "truncated" a quotation from that work to make it appear, falsely, that Mather rules out the efficacy of death-bed repentance. As to the first point, once again Geddes has invented the very object of his criticism and has then tried to make me bear the burden of responsibility for it: I simply never said, never even implied, that Mather's work was "a compendium of saintly deaths." As to the second point, Geddes claims that my contention is contradicted by the presence of the words "Change of Mind" and "usually no more than a conviction." The only way to deal with this charge is to quote in full the sentences in question along with the supporting sentences surrounding them, and let the reader decide: "Know, Finally, That a Repentance at the Last, if there be any *Space* for it, is mostly an *Insincere*, but always a *Suspicious* Repentance. . . . The *Change of Mind* [which is a synonym, Mather says earlier (p. 38) for repentance], which takes men upon a Death bed, is usually no more than a *Conviction* [meaning an awakened consciousness of sin] upon them; there is no *Real Conversion* in it. Men are then only like Iron softened in the *Fire*; they soon Return to their former Hardness if God spare them from going down into the *Unquenchable Fire*. . . . In an History of no less than *Four Thousand* years, there is but *One* Instance of a true *Repentance at the Last*, and he so circumstanced, as never any Sinner can be after him" (pp. 40-41).

Now if that does not pretty well "rule out the efficacy of death-bed repentance," then I do not know what does. I cannot, of course, tell

whether Geddes has intentionally misrepresented the meaning of this passage in an effort to justify the claim that my interpretation is not true to the sources, or whether he simply does not understand Mather's language. It is quite possible that the latter is the case, however, since he also does not seem to appreciate the difference between the modern sense of the word "acceptance" as used by Elisabeth Kübler-Ross, and the very complex meaning of the term "assurance" as it is employed in Puritan theology—despite what I had thought were almost didactic efforts on my part (especially pp. 1310–11) to make this point clear.

In the end Geddes states that "the Puritans were, then, traditional in their view of death." Obviously I disagree with that conclusion, but since Geddes has for some years been at work on a dissertation on Puritan attitudes toward death, I will welcome any positive contributions he may have to make to what are by no means closed questions. What I do not welcome are misrepresentations of my arguments and what can most charitably be described as unsupported allegations that my "use of sources is questionable."

DAVID E. STANNARD
Yale University

TO THE EDITOR:

In his review of *The Precarious Balance: English Government and Society* (*AHR*, 79 [1974]: 1546–47), Professor Hurstfield makes two very serious criticisms of my book: that it only occasionally makes students aware of diversity of opinion and that it apparently fails to deal justly with dissent. Together the charges add up to a larger one: that my book represents an "establishment interpretation" of the period.

For the first charge Hurstfield provides a piece of evidence, citing a "very odd footnote, on page 105." The point is well taken in that the note drops references to the whole controversy between Professor Elton and J. P. Cooper. Hurstfield must have realized this from the wrong wording of the reference he criticizes. There is no article by Elton entitled "Henry VII: Rapacity and Last Years Reconsidered." The note conflates Elton's original piece and the rebuttal by J. P. Cooper. Hence the sequence ought to have been: G. R. Elton, "Henry VII: Rapacity and Remorse," *HJ*, I (1958); J. P. Cooper, "Henry VII's Last Years Reconsidered," *HJ*, II (1959); and, last, G. R. Elton, "Henry VII: A Restatement," *HJ*, IV (1961). It is wide of the mark to make of this misprinted

note an intention to disguise from the reader a disagreement among historians. Readers will find no shortage of citations to conflicting views held among scholars in the first chapter notes and in every other set in the book.

There is no "establishment interpretation" at all at issue here.

Hurstfield raises another phantom in wrenching out of context a comment I made on page 107 of my book. He states that "the bland passage . . . which begins 'Freedom and the concrete liberties of which it consists are everywhere dependent on authority for vindication' (does authority never restrict or destroy liberty rather than vindicate it?) will leave an intelligent student wondering whether everything really was the best in the best of all possible worlds. It is, in this context, interesting to follow through Slavin's references to Thomas More." Hurstfield then goes on to raise the large question about my not being sufficiently skeptical about the "official viewpoint," which I suppose is almost as bad a fault as embracing the "establishment interpretation."

In fact the quotation is lifted, or ripped, from context. It occurs in a section of my book having nothing to do with dissent, which is treated sympathetically in whole chapters (see especially pp. 283–325). Instead, it occurs in the course of my attempt to show how important to ordinary people and their liberties the restoration of order was under Henry VII. After a lengthy discussion of monarchy, the aristocracy, kingcraft, finances, and law, order, and justice, I commented: "Freedom and the concrete liberties of which it consists are everywhere dependent on authority of vindication. More often than not in civilized societies, this authority takes the form of law. In England under York, and still more under the Tudors, Parliament made laws to bridge the gap between government as external and imposed on man and something inherent in the nature of social order itself." The comment is followed on the same page by this one: "The law needed new weapons to fight forces that had obstructed justice and dethroned its majesty. Ordinary men had experienced the lack of governance under Lancaster as an evil rather than a relief. They grasped eagerly at whatever promised to make law real and order an achievement rather than an aspiration. They were thus apt to allow expansions in the government's scope and methods, offering resistance only when rules accepted by the government seemed to strangle rights and offend against the order of local communities."

Perhaps here an issue is joined between Hurstfield and me, both in terms of canons of scholarship and partisanship and overall historical understanding. I would agree with him that "a history of the sixteenth century that cannot deal justly with dissent . . . presents a partial image of a complex and fascinating society." Perhaps he will agree with me, or at least readers of the review will agree with me, that I have dealt more justly with dissent in Tudor England than Hurstfield has dealt with my book, my ideas, and the range of my sympathies.

ARTHUR J. SLAVIN
University of Louisville

PROFESSOR HURSTFIELD REPLIES:

First the minor point. By his own account Dr. Slavin transposed part of Cooper's title on to an article by Elton with the result, as I wrote, that the reader has the reference to Elton's article and his reply and no reference to his critic, Cooper. But in the light of his explanation, I will at once accept that the omission was accidental. What, however, is disturbing is his assertion that the reviewer "must have realized this." I trust that this is not his normal method of reaching his conclusions. Let us say that his omission was just a Freudian slip and make no more of it.

More puzzling is his defense of the passage: "Freedom and the concrete liberties of which it consists are everywhere dependent on authority for vindication." His assertion here that it has been "ripped from context" is refuted by himself. It inaugurates a discussion about the early Tudors. That is why I commented on it precisely within its context. To tell us that he discusses dissent some two hundred pages further on (pp. 283-325), when dealing with a different period, reminds one of the presidential candidate who answered an awkward question at a meeting in Los Angeles, with the words: "I shall be dealing with that subject fully tomorrow night at Milwaukee." Whereupon the questioner called out: "But this is a major issue here in L.A.!" His discussion (long before p. 283) of Henry VIII's authority is conformist and has no reference to the central and vigorous discussion of his power, which has been going on for more than half a century.

But Slavin seems to have missed another fundamental point. In stating that liberties "are everywhere dependent on authority for vindication" he is using the present tense. It is a generalization about government. He is still

not asking himself whether so important and dogmatic a statement can in fact be established. Slavin clearly believes, as he has a right to, that the statement is true. I believe it to be untrue, whether applied to the Tudor period or our own. I shall indeed be delighted to leave the ultimate judgment to your readers.

JOEL HURSTFIELD
University College London

TO THE EDITOR:

In Dr. Clelia Duel Mosher's survey of her patients' sexual responses, Carl Degler, in his article "What Ought To Be and What Was: Women's Sexuality in the Nineteenth Century" (*AHR*, 79 [1974]: 1467-90), has found important new evidence regarding that complex, open issue, the nature of the American woman's sexuality in the nineteenth century. But Degler has been dismantling a nineteenth-century straw woman of his own devising. That nineteenth-century "civilized" sexual morality successfully suppressed the female orgasm or even tried to do so is an unnatural triumph I did not claim for it.

Indeed, I regarded the nineteenth-century norm as an attempt to "coerce a recalcitrant and hostile actuality." Degler and I cited the same physicians who opposed the extreme nineteenth-century emphasis on continence, argued that most women had moderate sexual appetites, and insisted that mutual satisfaction, clearly implying orgasm, was important for a happy marriage.

What I claimed was that after the 1840s a stronger premium was placed on continence, on saving sexual energies in the struggle for upward mobility. For several reasons, including the desire to limit offspring, marriages grew later and sexuality more "dangerous" for the aspiring middle classes. One result was a new emphasis on reticence as a way of controlling sexuality, especially among women. They were expected to remain innocent about sexual matters until marriage and were assumed to have less sexual desire than men. I suggested in passing that because of the widespread belief that orgasm led to conception, "coldness" may have been one mode of Malthusian control. I noted that some physicians observed an increase in "coldness" among women toward the turn of the century. Finally, I concluded that neurotics seemed to exhibit, as Freud suggested, conflicts between an unusually repressive internalized norm and their sexual drives, thus providing a ready-made clientele for the sexually-oriented unmasking therapy of psychoanalysis.

Mosher's survey in fact testifies to the successful impact of the nineteenth-century norm. Many of her patients directly linked controlling intercourse with controlling conception, which thirty out of forty-five regarded as the primary purpose of sex. One wished to bring children into the world only "under the most favorable circumstances"—precisely what I posited of upper-middle-class couples. Mosher's statistics also are revealing. To the question, "Do you always have a venereal orgasm during intercourse," nearly one-fifth replied "no, once or never." If these responses are added to those who replied "sometimes, not always or no with instances," the total reaches 59.9 per cent. Mosher blamed premarital ignorance for failures in adjustment, just as one might have expected her to do were the norm successfully instilled. Fifteen of her forty-five women thought intercourse unnecessary, while nine thought is a necessity for men only. The attitude of these women contrasts sharply with those expressed by women in that proliferation of studies of sexual behavior that began in the 1920s partly under the inspiration of psychoanalysis.

Finally, Mosher, a spinster and a member of the Dutch Reformed Church, herself was a strong believer in the core of the nineteenth-century code. A determined advocate of the physical equality of men and women, she advocated sublimation in order to reserve sexuality for the purposes of reproduction. In the third edition of *Woman's Physical Freedom* (1923), she noted that the increasing physical health of the modern young, also increased their sex urge. She proposed the conversion of this "primal force" into "creative work" through "wholesome worthwhile occupations" when sexuality was not needed for reproduction. "Thus are produced the ideally productive man and woman—the foundation of a successful democracy" (pp. 59–60).

NATHAN G. HALE, JR.
University of California,
Riverside

PROFESSOR DEGLER REPLIES:

Mr. Hale may believe that he and I have said the same things in our respective publications about the ideology and practices of women's sexuality in the nineteenth century, but I do not agree with him. He apparently believes that he made clear in his book that some writers of the nineteenth century "insisted that mutual satisfaction, clearly implying orgasm, was im-

portant for a happy marriage." But on page 31 of his book he wrote, "Many women came to regard marriage as little better than legalized prostitution. Sexual passion became associated almost exclusively with the male, with prostitutes, and women of the lower classes." It is not without significance, too, that although the discussion of female sexuality in his book contains several such references to the lack of female passion, not a single one of his quotations from medical writers of the period contain a direct reference to female orgasm.

For the same reason it is a little hard to accept his placing of Dr. Clelia Mosher in a class with Dr. William Acton and other proponents of passionless women. Mosher may not have been a "free spirit" of the 1920s, but when she asked her respondents if they "always had a venereal orgasm" she was hardly minimizing women's sexuality. Finally, the figures from the Mosher Survey, despite Hale's reinterpretation of them, still show that over three-quarters of the group experienced an orgasm, something that one would not learn from proponents of what Hale calls "'civilized' sexual morality" or from his discussion of the subject. In the end, of course, each reader, by reading our two accounts, will have to decide whether I have set up a "straw woman of my own devising."

CARL DEGLER
Stanford University

TO THE EDITOR:

It would seem advisable to redirect the attention of your readers to a work of Soviet scholarship, P. F. Laptin's *Obshchina v russkoi istoriografii poslednei treti XIX-nachala XX v.* [The Commune in Russian Historiography in the Last Third of the 19th and the Beginning of the 20th Century] (*AHR*, 80 [1975]: 143). In the *AHR* translation of the title the initial word was inadvertently rendered as "society" rather than "commune." Since the reviewer was allotted little space and used it to discuss other aspects of the work, readers unfamiliar with Russian are likely to remain unaware that the book deals specifically with the commune in the writings of a group of eminent Russian historians in the period indicated. At a time when controversy was mounting within the country on the historical role of the Russian commune, these exceptionally talented and methodologically innovative historians—I. V. Luchitskii, P. G. Vinogradov, M. M. Kovalevskii, A. N. Savin, and D. M. Petrushevskii—turned individually to an investigation of this basic social

institution as it had evolved outside Russia. Describing the commune as an institutional expression of a particular stage of universal social development whose course could be charted through a study of comparative history, they were to have considerable influence on Western historical thought and social theory (including Marxism in the case of at least one of these writers).

Recently the role of the commune in modern Russian history has been drawing increasing attention from Soviet and foreign scholars. Yet analysis of the historiography of the commune has been focused largely on the literature of the Populist-Marxist debate on the nature and future of the institution. Laptin's monograph (published by the Institute of History of the Ukrainian Academy of Sciences) is the first survey of the work on the commune of this important group of "liberal positivists" and as such, despite its limitations, may well be of interest to many readers.

DOROTHY ATKINSON
Stanford University

TO THE EDITOR:

I would like to correct possibly erroneous impressions left by Professor James Brundage's review of my book *Joscelyn III and the Fall of the Crusader States 1134-1199* (*AHR*, 79 [1974]: 1171-72).

My preface states the two goals of my book. First, "to present the increasingly interlocked Frankish-Selchük-Byzantine political and military relationships during the 1134-1199 period from a standpoint differing in varying degrees from the views appearing in the standard histories relative to this period of Near Eastern history." Second, "to show the involvement of Joscelyn III in the aforementioned military and political developments in general and the fall of the crusader states in particular and, in the course of so doing, solve and settle a number of controversies relating to his career."

Because Brundage misconstrued my purpose, he writes that I "tried . . . to bring the scanty evidence together [relative to the life of Joscelyn III] and to write a history of the Latin settlements in the East during Joscelyn's lifetime," and he says that I have failed to consult "much of the abundant recent literature on the society, economy, and institutions of the Latin states." My first goal connotes, I believe, an examination of the foreign policies identified with the Frankish, Selchük, and Byzantine groupings in respect to one another. It also connotes a con-

sideration of the internal political conditions in the entities within these groupings that constituted the dynamics of their foreign policies. Literature on the social, economic, and institutional aspects of these Latin states had little relevance to my goal.

Except for his reference to my failure to use recent scholarship on the battle of Hattin, an episode constituting seven pages in a book of 215 pages and three days in a life of sixty-five years (for which oversight I express my regrets), Brundage does not evaluate my second goal. Ninety-one of the 215 pages of this volume deal with the narrative of the life of Joscelyn III, and/or comments of other scholars on his life, and/or corrections of erroneous views on his life. I feel a reader of this review will have a false notion of the actual contents of my book. I would have welcomed evaluations of the narrative I have constructed of Joscelyn III's life, of the comments I have made on earlier scholarly treatment of this period, and of the problems about Joscelyn III I have tried to solve.

ROBERT L. NICHOLSON
University of Illinois,
Chicago Circle

TO THE EDITOR:

Kenneth V. Lottich's review of my *American Boarding Schools: A Historical Study* (*AHR*, 79 [1974]: 1255-56), seriously misrepresents the subject, scope, scholarship, and conclusions of that book.

Lottich writes: "McLachlan limits his field to fourteen down-East college preparatory schools," and he then goes on to list them. The book in fact deals in detail with six schools, two of them not mentioned on Lottich's list. He complains that I do not bring my account "to the current innovation in coeducation . . . nor greatly stress the recent urge to admit more black students and members of other minorities." Since the book plainly attempts to cover its subject only through the Progressive era, the omission of current affairs seems understandable and a peculiarly irrelevant ground for criticism.

One expects a scholarly review to place its subject within the literature of its field and to give some indication of the nature of the sources used in its preparation. Lottich does not mention that *American Boarding Schools* is the only history of that institution, or that it is based mainly on hitherto unexplored school archives and related manuscript collections. Instead, he suggests that the scholarship of the

work leaves something to be desired. He writes that "Sizer's *Age of the Academies* appears to have been little used or the popular historians of American education—Butts, Good, Meyer, Mulhern, or Commager—consulted." Sizer's slim collection of sources on academies is cited as often as its worth warrants. The other books "not consulted" are college textbooks on the history of American education, works of a type not usually cited in monographs based on primary sources. Moreover, H. S. Commager has never published a history, "popular" or otherwise, of American education.

Last, Lottich misrepresents my conclusions. He writes: "Quoting Domhoff it appears that the prime value of the select school is the inculcation of 'upper-class value, upper-class manners, and most of all upper-class speech.'" What I actually wrote suggests that the main concern of the book is radically different: "Rich parents did not send their sons to private boarding schools . . . to learn 'upper-class values, upper-class manners, and most of all upper-class speech.'" . . . Whatever their latent sociological function may have been, the manifest aim of these schools has always been to educate, not the aristocrat, but something quite different—the bourgeois gentleman." The book then goes on to explore this theme over a period of a century and makes no conclusions concerning class.

JAMES MCLACHLAN
Princeton University

TO THE EDITOR:

In his review of Norman C. Delaney's *John McIntosh Kell of the Raider Alabama* (AHR, 79 [1974]: 1265–66), Robert Seager II quotes the legendary Raphael Semmes: "My crew were never so happy as when they had plenty to do, and but little to think about. Indeed, as to thinking, I allowed them to do very little of that." Seager followed that quotation with the sweeping conclusion that "this view . . . is one that has not changed much among American naval officers since the Civil War." No facts are presented to support such a broad conclusion. It is my contention that even a cursory examination of the history of the United States Navy since the Civil War will reveal that the trend has been in exactly the opposite direction: the thinking man is the best man. The establishment of such technical ratings as the electronics technician and guided missileman as well as the many paths that lead from seaman

to admiral are some examples of this change in attitude.

To make such a broad, general assertion is equivalent to saying that historians, as a class, are polemicists who support their theses with arguments or opinions rather than facts just because some sail under the false colors of scholarly endeavor while engaged in such practice (see Warren F. Kimball's article, "The Cold War Warmed Over," in same issue).

I would be interested in the facts upon which Seager based his conclusion. Or is it another one of the many opinions regarding the military that have assumed the aura of fact because of repetition, and therefore common belief?

R. A. BOWLING
San Diego, California

The following exchange of letters has been received in connection with the publication of Warren Kimball's "The Cold War Warmed Over," AHR, 79 [1974]: 1119–36.

TO THE EDITOR:

You published an article by Warren Kimball on several cold-war books, including R. J. Maddox's egregious effort to smear the scholarly reputations and impugn the intellectual honesty of seven historians critical of official U.S. policies in the cold war. In the course of his article, Kimball dismisses my book *The Free World Colossus* as an "admittedly unscholarly account" and says that "the historical profession has rarely treated the book as a piece of objective research." He faults Maddox for even discussing it as a result. As his only support for this malicious assault, Kimball says that "of the others treated in Maddox's book, none used *The Free World Colossus* as a source," implying that even revisionists regard my work as unreliable and beneath their notice.

May I take the opportunity afforded by your "Communications" section to remove the knife from my back?

Of the six other revisionist works treated by Maddox, three were written before my book was published and therefore could not possibly have cited it as a source. Of the remaining three, Diane Clemens's *Yalta* is about an event that took place prior to the historical period analyzed in *The Free World Colossus*, which begins on April 23, 1945. Lloyd Gardner's *Architects of Illusion*—contrary to Kimball—cites and discusses *The Free World Colossus*, among several other interpretations of cold-war history. Finally,

although Gabriel Kolko's *The Politics of War* does not cite my book, its time span ends in 1945, and it also fails to cite William Appleman Williams and Gar Alperovitz, two revisionist authors whose work falls more appropriately within its historical frame, and whom Kimball seems to regard as scholarly and reputable. In short, although the method adopted by Kimball is a spurious way to determine whether a book is or ought to be regarded as objective research, it has the merit of showing how careless he is in his use of data and how disregarding of other historians' reputations.

For the record, my book—Maddox aside—has been generally viewed as an intellectually responsible account of America's foreign policy in the cold war. In John Lewis Gaddis's prize-winning *The United States and the Origins of the Cold War*—a book that Kimball reviews in the same essay, but which he has evidently not read very carefully—my book is referred to in the following context: "This [revisionist] interpretation was originally put forward by William A. Williams in *The Tragedy of American Diplomacy*. Important extensions and elaborations of Williams' thesis include Gardner, . . . ; Kolko, . . . ; LaFeber, . . . ; Alperovitz, . . . ; David Horowitz, *The Free World Colossus*" (p. 357n).

Finally, I wish to address myself to Kimball's repetition of Maddox's tendentious charge that my work is unscholarly because I failed to use many of the printed primary sources. Kimball knows the answer to this because he has read my detailed reply to Maddox (for copies write to Horowitz, 1319 Bonita, Berkeley, California 94709). It is in the nature of the task I originally set myself—to provide a systematic account of America's global policies from 1945 to 1963—that I could not write a history based on primary-source material. Most of the primary sources are still not available and some will never be. When I wrote the book, 1962–63, only a few volumes of the postwar *Foreign Relations* series cited by Maddox and Kimball were available. Anyone reading my brief references to Potsdam and Yalta would see that the information afforded by those collections was irrelevant to the issues raised in my text. The criterion of relevance is essential to the judgment of a historian's use of sources; otherwise, one would be in the position of having to refer to sources because they are primary, rather than because they are applicable to the issues at hand.

It is not easy for one who does not hold a university position to ply the historical trade

(although it has been a circumstance shared by many American historians in the past). I have been gratified on the one hand to have been asked to contribute the article "International Relations 1957–1971" to the new edition of the *Encyclopaedia Britannica* (indicating a general recognition of my contribution to postwar historical scholarship), just as I have been dismayed on the other, to be so shabbily treated in the pages of the *American Historical Review*.

DAVID HOROWITZ

Berkeley, California

TO THE EDITOR:

There is a piece of haughty nonsense in Warren F. Kimball's review article that inadvertently raises a serious question about history writing and also tells us a good deal about Professor Kimball's own attitudes.

Kimball airily dismisses David Horowitz's *Free World Colossus* in one paragraph as a "nonscholarly account," noting that the book was not reviewed in either the *American Historical Review* or the *Journal of American History*. He brushes off D. F. Fleming's *The Cold War and its Origins*, noting its purported "disappearance from recent bibliographies." There you have it: two important works of historical synthesis and reinterpretation and, poof! they become "unbooks" because they have failed to earn some specious credentials thought up by Kimball.

He rejects the Horowitz book on the additional grounds that the author "failed to use many of the printed primary sources." But doesn't the issue of the acceptability of his work hinge on the truthfulness of his account, the care with which he handles the assembled evidence, the degree to which evidence *not* used harms or supports his argument, the clarity and honesty of his presentation? Peculiarly enough, Kimball seems to recognize the truth that historical writing is necessarily a selective process of interpretation. Several times throughout his article he chides Maddox for ascribing "falsification of the evidence" to New Left historians when in fact what bothers Maddox is an interpretation different from his own. Kimball has not taken his own insight seriously enough in relation to Horowitz.

At the outset of his article, Kimball sketches a scenario of cold-war historiography, and diplomatic history in general, that winds up with the arrival of historians of the golden mean, *au-dessus de la mêlée*, who give us "eclectic synthesis." The question of whether "eclectic

synthesis" gives us a more veracious view of diplomatic reality aside, it would be interesting to know where Kimball locates himself in that scenario. Despite his pleas for the "genteel tradition," Kimball has, I think, slipped into the very partisanship he so piously derides.

LOUIS MENASHE

Polytechnic Institute of New York

TO THE EDITOR:

Professor Warren F. Kimball's own partisanship and faulty methodology are revealed, I believe, when he argues that Charles A. Beard and other revisionists isolated themselves by their "intemperance and nastiness." Kimball's argument is specious, moreover, when he claims that generations of postwar students might not have thought they had prostituted professional standards had they not conducted intemperate attacks on Franklin D. Roosevelt.

Kimball is merely echoing and repeating the very kind of vituperative slander and abuse heaped upon Charles A. Beard after 1948 by the academic and political establishments. Beard was isolated precisely because he dared to take on FDR, a president held sacrosanct. Beard's opponents moved rigorously to ostracize him from the profession and to ridicule his work. What upset them was that Beard criticized the methods used by Roosevelt to move the nation to war, the impulse of the New Deal to make the world over, and he also challenged the policies of deception carried out by the executive branch. Because deceptive policies were meant to bring the nation to war against an unpopular foe, most Americans, including American historians, proved unwilling or unable to examine Beard's arguments carefully.

Beard's arguments, whether one agrees with them or not, were stated carefully. His analysis was tightly constructed and logically presented; his case made with reason and lucidity. It was Beard, indeed, who was quite concerned about the decline of scholarly standards, demonstrated by the subsidy given William Langer and Everett Gleason by the Council on Foreign Relations, a grant that meant, Beard charged, that the council did not desire anyone "to examine too closely and criticize too freely" U.S. foreign policy. Attacking the special, privileged access to government documents given approved scholars, Beard concluded that "standards of comprehensive, balanced, and judicious scholarship have lost all value and appreciation in those quarters." If there was "intellectual beligerence" to be seen, it appeared in the various

responses to Beard's book—in the famous critique by Samuel Eliot Morison—and in the condemnation heaped upon Beard by Lewis Mumford, Allan Nevins, Henry Seidel Canby, and others.

Ironically, it is revisionist David Horowitz, writing without the academic scholar's credentials available to Warren F. Kimball, who has noted that what disturbed Beard's opponents was that his critique was set against the background of European fascism, and his "portrait of Roosevelt as the 'Caesaristic' betrayer of the American Republic served only to isolate him politically and bury his insights under an avalanche of abuse. Liberal and conservative historians joined together in ostracizing Beard and subjecting his work to scathing and patronizing attacks, which resulted in the rapid eclipse of his academic reputation and influence" (David Horowitz, "Historians and the Cold War," *Ramparts*, Aug.-Sept. 1973, pp. 36-40, 58-60).

Warren Kimball, evidently, prefers to keep up that very attack. Unlike Horowitz, he fails to understand that what is at stake is not a mere academic controversy, but division over a fundamental issue—the nature of America's role in the contemporary world. Beard was attacked because the academic establishment could not excuse his portrayal of the American government's imperial role, not because he levied "intemperate and personal attacks on Roosevelt." The contemporary revisionists, descendants of Beard, are attacked by those who still seek to delude themselves about that same role. In blaming the victim for his own victimization, Kimball has exonerated those who worked hard to read Charles A. Beard out of the profession and out of a dialogue about America's role. Perhaps we need more scholars who like Beard, and like David Horowitz, are unafraid to strongly state their case, even if in so doing, they depart from Kimball's cherished "genteel tradition."

RONALD RADOSH

*Queensborough Community College
and the Graduate Faculty,
City University of New York*

PROFESSOR KIMBALL REPLIES:

My primary interest in writing "The Cold War Warmed Over" was and continues to be a review of the three books listed and to provide an analysis of the broad thesis offered by Robert Maddox. My argument regarding the Maddox book rests upon the belief that mere footnote

analysis and insinuations of hypocrisy are an invalid and improper way to refute a scholarly interpretation. My decision to exclude David Horowitz's book from analysis stemmed not from some silly notions of academic snobbery but from my personal belief that *The Free World Colossus* is a competent lawyer's brief, reminiscent (in type though not in content) of the defenses of American foreign policy offered by the late John Foster Dulles. But Horowitz's thesis is not the reason for my attitude, as my remarks about other revisionist works demonstrate. Exercising what I presume is a scholar's prerogative, I judged his conclusions to be unsubstantiated by his evidence. I simply disagree with Horowitz's claim that the volumes on Yalta and Potsdam in the *Foreign Relations* series are "irrelevant to the issues" he raises. A comprehensive analysis of the scholarly reputation of a book (and I referred only to one specific work, not to Horowitz and his reputation) is difficult. Although the book obviously has its supporters, my impression of its overall reputation remains the same. (In this connection I should point out that Gaddis and Gardner do not cite *The Free World Colossus* as a source, which is what I wrote; they mention it only in a historiographical context.) Horowitz's inference that his nonacademic status somehow affected my opinion of his book is unfair and untrue.

Nor, to turn to Professor Menashe, do I consider Fleming's work an "unbook." Regardless of Fleming's historiographical significance, my point is that, as Maddox himself states that Fleming is not a New Left historian, Fleming's study should not have been included in a book titled *The New Left and the Origins of the Cold War*. Moreover, Fleming is outdated by more recent evidence, and I merely pointed out that it is easier to mount a superficial attack under those circumstances.

Ronald Radosh raises a separate issue that possesses much validity. My reference to "'white papers' style" was intended to be pejorative, though the point deserves reinforcing. Radosh's comments regarding Charles Beard add a perspective that I welcome, though I hasten to add

that he misinterprets my position. I agree that the opposition faced by World War II revisionists came from those who had a vested and/or official interest in protecting Franklin Roosevelt's reputation. They too occasionally transgressed the bounds of civility. I should have, at least briefly, mentioned the unsettling problem of histories written by those with special access privileges. Nevertheless, I am hardly keeping "up the attack" since I point out that those revisionists offered "insights, numerous hard and valid questions, and a potentially viable thesis." Radosh seems to contradict himself when he claims in his second paragraph that attacks against Beard occurred because "he dared to take on FDR" and then in his closing paragraph argues that Beard was not attacked because of his "'intemperate and personal attacks on Roosevelt.'" Beard undeniably held strong personal feelings about Franklin Roosevelt, and my argument related not to the substance of Beard's work but to the emotional commitment which I believe colored that work. Nor can Radosh defend the later actions of Charles Tansill or Harry Elmer Barnes as reasonable and scholarly. Barnes virtually deserted scholarship for rumor mongering and Tansill descended to emotional public denunciations of Roosevelt. The response of these and other World War II revisionists to those who attacked them may be understandable, but I believe that response was simultaneously self-defeating. My argument is not that Beard *et al.* deserved to have their reputations wrecked, but that they made it easier for their opponents to discredit them. As I pointed out in my essay, the late William Neumann made his points in a calm, gentle, scholarly manner—with lasting results.

Perhaps we need more scholars like Neumann, William A. Williams, Walter LaFeber, Lloyd Gardner, Barton Bernstein, Thomas Paterson, Gabriel Kolko, Gar Alperovitz, and others who are unafraid strongly to state their case, even if, in so doing, they do not depart from my cherished "genteel tradition."

WARREN F. KIMBALL
Rutgers University

Recent Deaths

When CHESTER HUGO KIRBY retired from active teaching in 1964, he enjoyed the satisfaction and the distinction of having devoted his entire professional career to the service of Brown University. Born in Sioux Falls, South Dakota, on September 12, 1899, he obtained his bachelor's and master's degrees from the State University of Iowa in 1921 and 1923. Moving on to Harvard, he won a second master's degree in 1924 and the Ph.D. in 1929, under the direction of the distinguished historian, Wilbur Cortez Abbott. In 1928 he married Ethan Williams who was an outstanding historian in her own right. During the previous year, he was appointed instructor in history at Brown University and thereafter thoroughly devoted himself to that institution, proceeding through the ranks of the academic hierarchy and retiring in 1964 after many years as full professor. In addition to his following on the campus, Kirby was well known in intellectual circles and served on the editorial board of the *Journal of Modern History* and the advisory board of the Delphian Society. He was also a member of the Authors Club in London. He died at Thetford, Vermont, on March 5, 1975.

Initially teaching European history, he was soon given the courses in English history, his specialty, in which he was extremely knowledgeable. Both Kirby and his wife, in fact, conducted extensive research in the British Museum. The economic and social aspects of modern English history were his forte, as he repeatedly demonstrated both in his teaching and in his many publications. Of these, the best known is his book, *The English Country Gentleman: A Study of Nineteenth Century Types*, which was published in London in 1936 and is still authoritative. Among his well-known articles is a remarkable series on the English game laws, which appeared in the *Journal of Modern History* (1932), the *American Historical Review* (1933), and the Harvard Press volume, *Essays*

on *Modern English History* (1941). These and his other publications attest to the breadth and expertise of his scholarship.

Chester Kirby will be remembered by many generations of Brown University students and his colleagues for the exacting standards that he set for himself and his pupils, both in his research and his teaching. He was particularly adept in handling small classes and discussion groups, long before they obtained their present vogue. His sense of humor was incisive, sometimes sardonic, sometimes trenchant, but always appropriate. And his personal dedication to historical learning and Brown University was complete.

WILLIAM F. CHURCH
Brown University

The death on November 21, 1974, of the Reverend Professor DAVID KNOWLES has deprived the historical profession of a leading authority on medieval monasticism and spirituality. Although he never visited the United States, his works were widely read here and have influenced the course of American scholarship. The fact, furthermore, that his first published book was *The American Civil War* (1926), in which he took a keen interest throughout his life, makes it especially fitting that he should be commemorated in the pages of this journal.

Professor Knowles was born in Studley, Warwickshire, on September 29, 1896, and was christened with the names Michael Clive. He was educated and became a monk at Downside, where he took the name David by which he was later known, and also at Cambridge, and in Rome, where he studied classics, theology, and philosophy before turning primarily to history. Professor Knowles was active in the monastery and school at Downside, as well as in scholarly research, from 1923 to 1933. He was then at Ealing Priory and in London before moving

in 1944 to Cambridge, where he was a fellow of Peterhouse and successively university lecturer, Professor of Medieval History, and Regius Professor of Modern History until he retired in 1963. After his retirement he lived in Wimbeldon and Liphook, Hampshire, where, in spite of failing health in recent years, he was active until almost the last day of his life.

As a person, Professor Knowles was a reserved and in some ways formidable figure whose gentle manner and frail appearance contrasted with his inner strength. Even his intimates were unacquainted with many aspects of his life, and it is in any case no concern of this notice to give an account of his activities as a monk and churchman, except to emphasize their interest and influence on his work as a historian.

His published writings cover a range of topics from the early church to contemporary affairs. His magnum opus is the history, in four volumes, of monasticism in England from the tenth to the sixteenth centuries (1940-59), with which are associated various specialized works and articles like the lists of medieval religious houses (1940; revised editions in 1953 and 1971) and their heads (1972), the edition of Lanfranc's *Decreta* (1951), and a survey of *Monastic Sites from the Air* (1952). Among his works on intellectual and spiritual history may be cited *The English Mystics* (1927), the well-known *Evolution of Medieval Thought* (1962), and the lectures on historiography published under the title *Great Historical Enterprises* (1963). These themes are brought together in his more general works on *The Benedictines* (1929), on *Christian Monasticism* (1969) and its constitutional history (*From Pachomius to Ignatius* [1966]), and on the history of the medieval church (1969), and also, in a different way, in his biographical works, such as the lecture (1949) and books (1951 and 1970) on Thomas Becket. A collection of his essays, with a curriculum vitae and bibliography, was published as a *Festschrift* in 1963 under the same title, *The Historian and Character*, as his inaugural lecture in 1955, where he expressed his conviction in the influence of individual personality on history.

No mere list of Professor Knowles's works can do justice, however, to their breadth of vision, balance of judgment, and charm and clarity of style, or to his influence as a teacher as well as a writer. When he was young, the history of monasticism and spirituality was the preserve of a comparatively small circle of scholars and was regarded by many Protestants with a distrust verging on dislike. The fact

that it is today regarded as a central aspect of the study of the Middle Ages is in large measure owing to the force and persuasiveness of Professor David Knowles.

GILES CONSTABLE
Harvard University

MARY ELIZABETH MASSEY, historian, teacher, and scholar, died on December 24, 1974, at Duke University Hospital. Born on December 25, 1915, in Morrilton, Arkansas, she received the B.A. degree from Hendrix College and the M.A. and Ph.D. degree from the University of North Carolina at Chapel Hill.

After teaching at Morrilton High School and serving as director of the Hendrix College Training School, Dr. Massey taught at Flora Macdonald College and later at Washington College. In 1950 she joined the faculty of Winthrop College where she chaired the department of history from 1960 to 1964. While at Winthrop she received numerous student awards for outstanding teaching and was selected distinguished professor in 1965 and faculty representative on the board of trustees in 1972.

During her career she held research awards from the Southern Fellowship Fund, the Huntington Library, and the Guggenheim Foundation. She wrote numerous scholarly articles, but is best known for her three books about the Civil War: *Ersatz in the Confederacy* (1952), *Refugee Life in the Confederacy* (1964), and *Bonnet Brigades: Women and the Civil War* (1966).

Dr. Massey was a member of the South Carolina Commission of Archives and History (1962-63), the Advisory Council of the National Civil War Centennial Committee (1961-65), the South Carolina Civil War Centennial Commission (1961-65), the South Carolina Tri-Centennial Commission (1968-71), and at her demise was a member of the board of advisers of the National Historical Society. Of the professional recognition which Dr. Massey attained, she cherished most the offices that she held in the Southern Historical Association, serving as its president in 1971-72.

Professor Massey was the first woman to address the New Orleans Civil War Round Table, the only woman to contribute to the fifteen-volume Impact Series on the Civil War, the first woman to receive the Distinguished Alumna Award from Hendrix College (1967), and the third woman to hold the presidency of the Southern Historical Association. Mary Elizabeth Massey appreciated the role of women in his-

tory and served their cause faithfully, if not militantly. Before her death she completed a paper on Mary Todd Lincoln that was read for her before the National Historical Society. The historical profession will miss her brilliance, her lively sense of humor, and, above all, her devotion to Clio.

Memorials in her name may be made to the history department of Winthrop College.

ROSS A. WEBB
Winthrop College

PIERRE RENOUVIN, France's most eminent historian of international relations and an honorary member of the American Historical Association since 1947, died in Paris on December 8, 1974, at the age of eighty-one. His remarkable career as scholar, teacher, and editor spanned more than a half century. Born in the Latin Quarter on January 8, 1893, he earned degrees in both law and history and became an agrégé in history at the age of nineteen. Mobilized in 1914 as a lieutenant of infantry, he was gravely wounded during the Nivelle offensive of 1917 and suffered the loss of his left arm. Midway through his *thèse de doctorat* (on the provincial assemblies of 1787), the ministry of education in 1920 chose him to direct an official study of the Great War—a call that diverted him into diplomatic and contemporary history. His official and academic responsibilities grew by steady accretion. He headed the Bibliothèque de Documentation Internationale Contemporaine from 1920 to 1936; he became secretary general of the *Revue d'histoire de la guerre mondiale* and of the publications committee for the *Documents diplomatiques français, 1871-1914*; he taught at the Sorbonne from 1922 until his retirement in 1964, and at the Ecole des Sciences Politiques (as well as its post-war reincarnation) from 1938 onward. After the second war he assumed still other burdens: coeditor and then editor of the *Revue historique* (1942-74); chief editor of the new *Documents diplomatiques français, 1932-1939*; doyen at the Sorbonne from 1955 to 1958; president of the Fondation Nationale des Sciences Politiques. With the multiplication of duties came a steady flow of honors: membership in the Institut, Grand Cross of the Legion of Honor, and others too numerous to list.

Despite these manifold responsibilities, Renouvin found time and energy to publish more than a dozen authoritative books and scores of articles. Between the wars he focused mainly on the origins and impact of the Great War;

his stance was moderately antirevisionist, but his work was respected in all camps. Thereafter his interests broadened to encompass the whole of modern international relations. He edited a seven-volume *Histoire des relations internationales* (1953-58), of which he wrote the last four volumes; and together with his former student and Sorbonne successor J.-B. Duroselle he published an *Introduction à l'histoire des relations internationales* (1964). Although he eschewed theory and methodology, these works implicitly placed him somewhere between the traditional narrative or "eventish" historians and the rising *Annales* school; he was concerned to probe the deeper forces, material and moral, that lead men to act or that channel their actions.

Honors and successes left Renouvin untouched. There was nothing of the prima donna about him, in or out of the classroom. He was a skillful and influential teacher who drew a large, attentive following; his lucid and forceful lectures were lightly tinged with irony. He had little taste for the factional feuds of French academic life. He was forthright and unpretentious in manner, generous and helpful in spirit. Dozens of American scholars, both callow and mature, found him ready to take them seriously and to offer wise counsel. Scores of French scholars were trained in his seminars and looked to him as their mentor and inspirer. He made no effort to attract disciples; perhaps that is why he had so many. Only ten days before his death, he presided for five hours over a thesis jury with his customary skill and authority. On his desk was the manuscript of still another book, almost completed, on French public opinion in 1917. He remained, from his earliest years until the very end, a true professional.

GORDON WRIGHT
Stanford University

KENNETH WALLACE COLEGROVE, the first Distinguished University Professor of History and Political Science at Long Island University and the first senior research associate at the Center for the Study of the Presidency, died in New York on January 3, 1975.

Born in Waukon, Iowa, on October 8, 1886, he was the son of the president of Upper Iowa University and a graduate of both the University of Northern Iowa and the State University of Iowa. Subsequently Colegrove was a student at Harvard of two of the nation's giants in the field of history, Edward Channing and

Frederick Jackson Turner. Receiving his Ph.D. degree in history at Harvard in 1915, he had two years before already begun his teaching career in history at Mount Holyoke. He was, at the time of his death, one of the most senior members of the American Historical Association. Throughout his career he combined in his teaching and writings, in his inimitable manner, the disciplines of history, philosophy, and political science.

After three years at Mount Holyoke College as a lecturer, Colegrove served as an assistant professor of history at Syracuse University for three years. Then in 1919 he began his thirty-three-year teaching career at Northwestern University in political science, retiring in 1952. During much of this period he served as chairman of the department and also as secretary-treasurer of the American Political Science Association.

Colegrove's career was characterized by meritorious work both in scholarship and public service. For more than a generation his counsel was sought at the highest level in the nation's capital. During World War II he served as a consultant to the Office of Strategic Services. Following the war, as General Douglas MacArthur's political adviser, Colegrove drafted the Japanese Constitution. His public service also included a special role with former President Herbert Hoover and with Presidents Truman and Eisenhower during the Hoover commissions on government organization and with the Hoover libraries both at Stanford University and at West Branch, Iowa. A defender of individual liberty, he instituted what became a landmark Supreme Court decision in the case of *Colegrove v. Green*, 1946, in which, in quest of fair apportionment, he championed the principles of just representation under law.

Following his retirement from Northwestern, after teaching a year in City University of New York, Colegrove accepted the challenge offered by Long Island University to develop the graduate programs in history and political science at its new C. W. Post Campus. He served there from 1956 to 1969. These productive second-career years, 1952-69, also included his writings on democracy versus communism and related studies both for schools and the armed forces, an advanced course for the Naval War College, and a counsellor role with the Reim Foundation.

Following his second retirement in 1969, Colegrove, as an octogenarian began a third career, serving as the first senior research associate at the Center for the Study of the Presi-

dency until his death. At the center he helped to launch the *Presidential Studies Quarterly*. Here, as in other institutions in which he so brilliantly served, his work will be an inspiration to future generations of students.

R. GORDON HOXIE

Center for the Study of the Presidency

DONALD GROVE BARNES died in Cleveland on February 14, 1975, at the age of eighty-two. A native of Albion, Nebraska, he was educated at the University of Nebraska and at Harvard. As a young man he taught briefly at the universities of Oregon and Washington. In 1934 he joined the faculty of Western Reserve University, where he remained for twenty-eight years, until his retirement in 1962.

Professor Barnes's field of concentration was eighteenth-century English history. His two major works, *History of the English Corn Laws from 1660-1846* (1930) and *George Third and William Pitt, 1783-1806* (1965), were heralded for their outstanding scholarship when they appeared, and are still regarded as the standard works, while his later studies on Newcastle and Cornwallis, published as articles or presented as papers at conferences, were equally admired for their documentation and their penetrating analysis.

Besides being honored as a distinguished scholar with a Guggenheim fellowship, membership in the Royal Historical Society, and the presidency of the Conference on British Studies, 1965-67, Donald Barnes was well known and loved on both sides of the Atlantic for his genial personality. A consummate *bon vivant* and raconteur, he was sought as much for his good company as he was admired for his excellent scholarship, and for the loss of both he will be greatly missed.

RUTH EMERY

Rutgers University

WALTER VINTON SCHOLES, professor of history at the University of Missouri—Columbia, died after an extended illness on January 24, 1975, at the age of fifty-eight. Born in Bradford, Illinois, on July 26, 1916, he received his B.A., M.A., and Ph.D. degrees from the University of Michigan. He taught at Stephens College, Columbia, Missouri, from 1943 to 1945 and then moved across town to the University of Missouri where he taught until his untimely death. Scholes served as department chairman from 1953 to 1956 and was a visiting professor at

the University of Wisconsin, Madison, in 1968–69. Trained as a Latin Americanist, he published several works on Mexico, including *The Diego Ramirez Visita* (1946) and *Mexican Politics during the Juárez Regime* (1957), and served on the board of editors of the *Hispanic American Historical Review* (1953–57) and as a contributing editor of the *Handbook of Latin American Studies* (1952–55). Later in his career his interests shifted to United States diplomatic history. Among his publications in this field were *The Foreign Policies of the Taft Administration* (1970), which he coauthored with his wife, Marie Vielmetti Scholes, and *United States Diplomatic History* (1973). At the time of his death he was working on a book on the foreign policy of Charles Evans Hughes. Scholes con-

veyed to his students his own excitement about his subject: his lecture courses were always popular and at his death he was handling the heaviest load of doctoral candidates in the department.

WINFIELD J. BURGGRAEFF
*University of Missouri—
Columbia*

We apologize to the family of BERT J. LOEWENBERG and to Dean Carl Resek for the transposed lines that appeared in Dean Resek's obituary of Mr. Loewenberg, in our June issue, page 746. This unfortunate error occurred at the printing plant after the proofs had been read. The editors deeply regret any confusion that may have resulted.

Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

ALEXANDER, THOMAS G., editor. *Essays on the American West, 1973-1974*. (Charles Redd Monographs in Western History, No. 5.) Provo, Utah: Brigham Young University Press. 1975. Pp. v, 131. \$3.95.

LEROY R. HAFEN, The Opening and Development of the First Route from the Rockies to the Pacific. RICHARD H. JACKSON, Righteousness and Environmental Change: The Mormons and the Environment. S. LYMAN TYLER, The Recent Urbanization of the American Indian. TED J. WARNER, The Significance of the Domínguez-Vélez de Escalante Expedition. JEAN BICKMORE WHITE, Woman's Place Is in the Constitution: The Struggle for Equal Rights in Utah in 1895. KARL E. YOUNG, Charles Redd: Profile of a Renaissance Man as Rancher.

AUTY, ROBERT, *et al.*, editors. *Oxford Slavonic Papers*. (New Series, Volume 7.) New York: Oxford University Press. 1974. Pp. 123. \$12.00.

ROBERT AUTY, B. O. Unbegaun's Contributions to Russian and Slavonic Philology. J. LURIA (YA. S. LUR'E), An Unpublished Epigram on an English Ambassador to Russia. MARC RAEFF, The Empress and the Vinerian Professor: Catherine II's Projects of Government Reforms and Blackstone's *Commentaries*. A. H. BROWN, S. E. Desnitsky, Adam Smith, and the *Nakaz* of Catherine II. SERGIUS YAKOBSON, Richard Cobden's Sojourn in Russia, 1847. W. HARRISON, The British Press and the Russian Revolution of 1905-1907. T. J. BINYON, Valery Bryusov and the Nature of Art. J. L. I. FENNELL, The Struggle for Power in North-east Russia, 1246-9: An Investigation of the Sources.

Camden Miscellany Vol. XXVI. (Camden Fourth Series, Volume 14.) London: Royal Historical Society. 1975. Pp. 213. £3.50.

ROBERT SOMERVILLE (editor), Ordinances for the Duchy of Lancaster. P. R. ROBERTS (editor), 'A Breviat of the Effectes Devised for Wales'. c. 1540-41. CHARLES L. HAMILTON (editor), 'The Muster-Master' by Grevasse Markham. LAWRENCE SQUIBB and WILLIAM HAMILTON BRYSON (editor), A Book of All the Several Officers of the Court of Exchequer Together with the Names of the Present Officers, in Whose Gift, and How Admitted. With a Brief Collection of the Chief Heads of What Every Officer Usually Doeth by Vertue of His Office According to the State of the Exchequer at this Day, January 1641. H. T. DICKINSON (editor), The Letters of Henry St. John to the Earl of Orkney 1709-1711.

HEICK, W. H., and GRAHAM, ROGER, editors. *His Own Man: Essays in Honour of Arthur Reginald Marsden Lower*. Montreal: McGill-Queen's University Press. 1974. Pp. ix, 187. \$7.50.

FREDERICK W. GIBSON, Arthur Lower: Always the Same and Always His Own Man. MARGARET PRANG, A. R. M. Lower: The Professor and "Relevance." W. H. HEICK, The Character and Spirit of an Age: A Study of the Thought of Arthur R. M. Lower. RAMSAY COOK, Canadian Freedom in Wartime 1939-1945. W. L. MORTON, Further Reflections on the "Most Famous Stream." H. PEARSON GUNDEY, Liberty and Licence of the Press in Upper Canada. S. F. WISE, Sport and Class Values in Old Ontario and Quebec. BRUCE W. HODGINS, Unconventional Priest of the North: Charles Paradis, 1848-1926. D. C. MASTERS, Jasper Nicolls and English Protestant Education in Canada East. ANNE MACDERMAID (compiler), A Selected List of Publications by A. R. M. Lower. ANNE MACDERMAID (compiler), The A. R. M. Lower Papers: An Inventory.

JUNKER, BEAT, *et al.*, editors. *Geschichte und politische Wissenschaft: Festschrift für Erich*

Gruner zum 60. Geburtstag. Bern: Francke Verlag. 1975. Pp. 372.

BEAT JUNKER, Die Bundesfeier als Ausdruck nationalen Empfindens in der Schweiz um 1900. DANIEL FREI, Überlegungen zum Stellenwert des Nationalen in der Weltpolitik. ULRICH IM HOF, Die Viersprachigkeit der Schweiz als Minoritätenproblem des 19. und 20. Jahrhunderts. ERNEST WEIBEL, La problématique des minorités, le pluralisme suisse et le cas du Tessin. ROLAND RUFFIEUX, Les groupes de pression et la démocratie semi-directe en Suisse. KLAUS VON BEYME, Neuere Ansätze zur Theorie des Gruppenpluralismus. ANDRÉ LASSERRE, Pourquoi être socialiste dans le canton de Vaud en 1914? ROGER GIROD, Situation de fortune, statut social et parti à Genève à la fin des années 1960. PAUL H. EHINGER, Links und rechts als Begriffe der politischen Theorie und Praxis. CHRISTOPHER HUGHES, Violence and Non-Violence in Swiss Constitutional Amendment. KURT EICHENBERGER, Systemwahrende Kontinuität in Verfassungsänderungen. MARKUS MATTMÜLLER, Die Durchsetzung des allgemeinen Wahlrechts als gesamteuropäischer Vorgang. GERHARD LEHMBRUCH, Die ambivalenten Funktionen politischer Beteiligung in hochindustrialisierten Demokratien. GEORGES ANDREY, Personnalité politique et carrière parlementaire: Aspects de la longévité de fonction au Conseil national suisse sous le régime majoritaire (1848 à 1918). LEONHARD NEIDHART, Repräsentationsformen in der direkten Demokratie: Aspekte des schweizerischen Staatsbildungsprozesses. KURT B. MAYER, Amerikanische Untersuchungen zur Machtstruktur der Gemeinde.

ULRICH KLÖTI, Bibliographie der Publikationen Erich Gruners.

O'DONNELL, J. REGINALD, editor. *Essays in Honour of Anton Charles Pegis*. Toronto: Pontifical Institute of Mediaeval Studies. 1974. Pp. 395. \$18.00.

J. REGINALD O'DONNELL, Anton Charles Pegis on the Occasion of His Retirement. WILLIAM J. CONLAN, O.P., The Definition of Faith According to a Question of MS. Assisi 138: Study and Edition of Text. PAUL VINCENT SPADE, Five Logical Tracts by Richard Lavenham. ARMAND MAURER, C.S.B., Henry of Harclay's Disputed Question on the Plurality of Forms. VIRGINIA BROWN, Giovanni Argiropulo on the Agent Intellect: An Edition of Ms. Magliabecchi V 42 (ff. 224-228^v). EDWARD A. SYNAN, *The Exortacio* Against Peter Abelard's *Dialogus inter Philosophum, Iudaeum et Christianum*. WILMA FITZGERALD, *Nugae Hyginianae*. MICHAEL M. SHEEHAN, Marriage and Family in English Conciliar and Synodal Legislation. LAURENCE K. SHOOK, Riddles Relating to the Anglo-Saxon Scriptorium. LEONARD E. BOYLE, O.P., *The De Regno* and the Two Powers. EDMUND COLLEDGE, O.S.A., A Middle English Christological Poem. MICHAEL R. E. GOUGH, Three Forgotten Martyrs of Anazarbus in Cilicia. NIKOLAUS HÄRING, Chartres and Paris Revisited. WALTER HAYES, *Greek Recentiores*, (Ps.) Basil, *Adversus Eunomium*, IV-V. JOSEPH OWENS, C.S.S.R., The Physical World of Parmenides.

Other Books Received

Books listed were received by the AHR between February 1 and May 1, 1975. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

- BEAUMONT, ROGER A., and EDMONDS, MARTIN (eds.). *War in the Next Decade*. [Lexington:] University Press of Kentucky. 1975. Pp. x, 217. \$11.00.
- BENNIGSEN, ALEXANDRE. *Russes et Chinois avant 1917*. Questions d'histoire, 34. Paris: Flammarion. 1974. Pp. 185.
- BICKEL, RICHARD B. (comp.). *Manuscripts on Microfilm: A Checklist of the Holdings in the Manuscript Division*. Washington: Library of Congress. 1975. Pp. 82. \$1.15.
- CAMPBELL, GEORGE F. *China Tea Clippers*. New York: David McKay. 1975. Pp. 156. \$12.95.
- CORNFORD, JAMES (ed.). *The Failure of the State: On the Distribution of Political and Economic Power in Europe*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 198. \$17.50.
- CUNNINGTON, PHILLIS. *Costume of Household Servants: From the Middle Ages to 1900*. New York: Barnes & Noble. 1975. Pp. ix, 165. \$19.50.
- FAIRBANK, JOHN K. *Chinese-American Interactions: A Historical Summary*. 1974 Brown and Haley Lectures. New Brunswick: Rutgers University Press. 1975. Pp. 90. Cloth \$6.50, paper \$2.95.
- General Inventory Manuscripts*. Vol. 7, MG 29. Ottawa: Public Archives of Canada, Manuscript Division. 1975. Pp. viii, 149. \$3.60.
- GREEN, DAVID. *Blenheim*. New York: Charles Scribner's Sons. 1975. Pp. 162. \$10.00.
- HALL, RICHARD. *Stanley: An Adventurer Explored*. Boston: Houghton Mifflin. 1975. Pp. 400. \$12.50.
- JOLL, JAMES. *The Second International 1889-1914*. Rev. ed.; Boston: Routledge and Kegan Paul. 1975. Pp. viii, 224. \$11.75.
- MCDUGALL, MARY LYNN. (ed. with an introd.). *The Working Class in Modern Europe*. Problems in European Civilization Ser. Lexington, Mass.: D. C. Heath. 1975. Pp. xvii, 185.
- MCINNIS, RAYMOND G., and SCOTT, JAMES W. *Social Science Research Handbook*. New York: Barnes & Noble. 1975. Pp. xix, 395. \$10.00.
- MECKLER, ALAN M., and McMULLIN, RUTH (comps. and eds.). *Oral History Collections*. New York: R. R. Bowker. 1975. Pp. 344.
- MIGONE, GIAN GIACOMO. *Problemi di storia nei rapporti tra Italia e Stati Uniti: Corso di storia dell'America Settentrionale*, Università di Torino, anno accademico 1970-1971. Turin: Rosenberg & Sellier. 1971. Pp. v, 156. L. 2,200.
- PATERSON, J. H. *North America: A Geography of Canada and the United States*. 5th ed.; New York: Oxford University Press. 1975. Pp. xiii, 364. \$9.50.
- POIDEVIN, RAYMOND. *Les origines de la première guerre mondiale*. Documents Histoire, 11. [Paris:] Presses Universitaires de France. 1975. Pp. 118.
- PRINCE, MORTON. *Psychotherapy and Multiple Personality: Selected Essays*. Ed. with introductory essay by NATHAN G. HALE, JR. Cambridge, Mass.: Harvard University Press. 1975. Pp. vi, 328. \$12.50.
- REMMLING, GUNTER W. *The Sociology of Karl Mannheim: With a Bibliographical Guide to the Sociology of Knowledge, Ideological Analysis, and Social Planning*. Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. xv, 255.
- SANDERSON, MICHAEL. *Sea Battles: A Reference Guide*. Middletown, Conn.: Wesleyan University Press. 1975. Pp. 199. \$9.95.
- STAVRIANOS, L. S. *The World to 1500: A Global History*. 2d ed.; Englewood Cliffs, N.J.: Prentice-Hall. 1975. Pp. 399.
- STEARNS, PETER N. *European Society in Upheaval: Social History since 1750*. 2d ed.; New York: Macmillan. 1975. Pp. xii, 360. \$6.95.
- STUART, GRAHAM H., and TIGNER, JAMES L. *Latin America and the United States*. 6th ed.; Englewood Cliffs, N.J.: Prentice-Hall. 1975. Pp. viii, 856.

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- GIACCHERO, MARTA (ed.). *Edictum Diocletiani et Collegarum de pretiis rerum venalium in integrum fere restitutum e Latinis Graecisque fragmentis*. Vol. 1, *Edictum*; vol. 2, *Imagines*. Pubblicazioni dell'Istituto di Storia Antica e Scienze Ausiliarie dell'Università di Genova. Genoa: the Istituto. 1974. Pp. viii, 313; 177.
- KAGAN, DONALD. *Problems in Ancient History*. Vol. 1, *The Ancient Near East and Greece*; vol. 2, *The Roman World*. 2d ed.; New York: Macmillan. 1975. Pp. x, 480; x, 453. \$5.95; \$6.95.
- PEROWNE, STEWART. *The Caesars' Wives: Above Suspicion?* London: Hodder and Stoughton; distrib. by Rowman and Littlefield, Totowa, N.J. 1974. Pp. 192. \$7.50.
- PRICE, DEREK DE SOLLA. *Gears from the Greeks: The Antikythera Mechanism—A Calendar Computer from ca. 80 B.C.* New York: Science History Pub-

- lications; distrib. by Neale Watson Academic Publications, New York. 1975. Pp. 70. \$8.50.
- SCHLATTER, RICHARD (ed. with an introd.). *Hobbes's Thucydides*. New Brunswick: Rutgers University Press. 1975. Pp. xxviii, 587. Cloth \$17.50, paper \$6.95.
- TUDOR, D. *Les ponts Romains du Bas-Danube*. Bibliotheca Historica Romaniae Études, 51. Bucharest: Editura Academiei Republicii Socialiste România. 1974. Pp. 175. Lei 9.

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- BASIN, THOMAS. *Apologie ou plaidoyer pour moi-même*. Ed. and tr. by CHARLES SAMARAN and GEORGETTE DE GROËR. Les classiques de l'histoire de France au Moyen Âge, vol. 31. Paris: Société d'Édition "Les Belles Lettres." 1974. xii, 285.
- BROOKE, CHRISTOPHER. *Europe in the Central Middle Ages 962-1154*. A General History of Europe Ser. Rev. ed.; New York: Longman. 1975. Pp. xix, 493. \$7.95. See rev. of 1st ed. (1964), *AHR*, 70 (1964-65): 841.
- CHRIMES, S. B., et al. (eds.). *Fifteenth-Century England, 1399-1509: Studies in Politics and Society*. New York: Barnes & Noble. 1972. Pp. 192. \$10.00.
- GIBB, HAMILTON A. R. *Saladin: Studies in Islamic History*. Ed. by YUSUF IBISH. Beirut: Arab Institute for Research and Publishing; distrib. by International Scholarly Book Services, Portland, Ore. 1975. Pp. viii, 210. \$10.00.
- HEER, FRIEDRICH. *Charlemagne and His World*. New York: Macmillan. 1975. Pp. 272. \$15.00.
- LARÈS, MICHELINE M. *Bible et civilisation anglaise: Naissance d'une tradition (Ancien Testament)*. Publications de la Sorbonne, Littératures, no. 6. Paris: Didier. n.d. Pp. 345.
- LEA, HENRY CHARLES. *The Duel and the Oath*. With additional original documents in tr. by ARTHUR C. HOWLAND. Ed. with an introd. by EDWARD PETERS. Philadelphia: University of Pennsylvania Press. 1974. Pp. 259. \$10.00.
- PAUL THE DEACON. *History of the Lombards*. Tr. by WILLIAM DUDLEY FOULKE. Ed. with an introd. by EDWARD PETERS. Reprint; Philadelphia: University of Pennsylvania Press. [1974.] Pp. xxi, 329. \$12.50.
- SMALLEY, BERYL. *Historians in the Middle Ages*. New York: Charles Scribner's Sons. 1975. Pp. 202. \$12.50.
- WEIBULL, CURT. *Die Geaten des Beowulfepos und Die dänischen Trelleburgen: Zwei Diskussionsbeiträge*. Acta Regiae Societatis Scientiarum et Litterarum Gothoburgensis. Humaniora, 10. Göteborg: Kungl. Vetenskaps-och Vitterhets-Samhället. 1974. Pp. 43.
- WILLIMAN, DANIEL. *Records of the Papal Right of Spoil 1316-1412*. Institut de Recherche et d'Histoire des Textes. Paris: Centre National de la Recherche Scientifique. 1974. Pp. viii, 254.

BRITISH COMMONWEALTH AND IRELAND

- BLAKELY, BRIAN L., and COLLINS, JACQUELIN (comps.). *Documents in English History: Early Times to the Present*. New York: John Wiley & Sons. 1975. Pp. xii, 467.
- SAINTY, J. C. (comp.). *Office-Holders in Modern Britain*. Vol. 4. *Admiralty Officials, 1660-1870*.

London: University of London, Institute of Historical Research, Athlone Press; distrib. by Humanities Press, Atlantic Highlands, N.J. 1975. Pp. xiv, 161. \$19.50.

- SMITH, GADDIS. *Britain's Clandestine Submarines 1914-1915*. Reprint; [Hamden, Conn.]: Archon Books. 1975. Pp. vi, 155. \$7.50. See rev. of 1st printing (1964), *AHR*, 70 (1964-65): 860.
- TREASURE, G. R. R. *Who's Who in History*. Vol. 5. *England 1789-1837*. New York: Barnes & Noble. 1975. Pp. xii, 413. \$21.50.
- UNITED KINGDOM. OECD Economic Surveys. [Washington:] Organisation for Economic Cooperation and Development. 1975. Pp. 49. \$1.75.
- WILLIAMSON, AUDREY. *Wilkes: 'A Friend to Liberty.'* New York: Reader's Digest Press; distrib. by E. P. Dutton. 1974. Pp. 250. \$10.00.

FRANCE

- BERCÉ, YVES-MARIE. *Croquants et Nu-pieds: Les soulèvements paysans en France du XVI^e au XIX^e siècle*. Collection Archives, 55. Paris: Gallimard/Julliard. 1974. Pp. 240.
- BLONDEL, J. *Contemporary France: Politics, Society and Institutions*. Rev., expanded and reprinted from *France: A Companion to French Studies*. Ed. by D. G. CHARLTON. London: Methuen; distrib. by Barnes & Noble, New York. 1975. Pp. viii, 91. \$3.75.
- DÉMARET, PIERRE, and PLUME, CHRISTIAN. *Target de Gaulle: The True Story of the 31 Attempts on the Life of the French President*. Tr. by RICHARD BARRY. New York: Dial Press. 1975. Pp. 293. \$10.00.
- GREENLAW, RALPH W. (ed. with an introd.). *The Social Origins of the French Revolution: The Debate on the Role of the Middle Classes*. Problems in European Civilization. Lexington, Mass.: D. C. Heath. [1975.] Pp. xxi, 259.
- MAILET DU PAN, JACQUES. *Considerations on the Nature of the French Revolution: And on the Causes which Prolong Its Duration*. With an introd. by PAUL H. BEIK. New York: Howard Fertig. 1974. Pp. xxi, 114. \$11.75.
- MÉTHIVIER, HUBERT. *La France de Louis XIV: Une grand règne? Documents Histoire*, 12. [Paris:] Presses Universitaires de France. 1975. Pp. 141.
- MORIN, ALFRED. *Catalogue descriptif de la Bibliothèque Bleue de Troyes*. Almanachs exclus. Centre de Recherches d'Histoire et de Philologie de la IV^e Section de l'École pratique des Hautes Etudes, Histoire et civilisation du livre, 7. Geneva: Librairie Droz. 1974. Pp. 501.
- POTTS, D. C., and CHARLTON, D. C. *French Thought since 1600*. Rev. and reprinted from *France: A Companion to French Studies*. Ed. by D. G. CHARLTON. London: Methuen; distrib. by Barnes & Noble, New York. 1975. Pp. viii, 96. \$3.75.
- TAINÉ, HIPPOLYTE ADOLPHE. *The Origins of Contemporary France: The Ancient Regime, The Revolution, The Modern Regime*. Ed. with introd. by EDWARD T. GARGAN. Classic European Historians Ser. Chicago: University of Chicago Press. 1974. Pp. xlv, 446. \$15.00.
- TEILHARD DE CHARDIN, PIERRE. *Toward the Future*. Tr. by RENÉ HAGUE. A Helen and Kurt Wolff Book. New York: Harcourt Brace Jovanovich. 1975. Pp. 224. \$6.95.

VILLARS, JEAN BERAUD. *Notes of a Lost Pilot*. Tr. and ed. with foreword and notes by STANLEY J. PINCELT, JR. and ERNEST MARCHAND. [Hamden, Conn.:] Archon Books. 1975. Pp. 285. \$12.50.

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JADOS, STANLEY S. *Consulate of the Sea and Related Documents*. University: University of Alabama Press. 1975. Pp. xxxii, 326. \$12.00.

THEMUDO BARATA, MARIA DO ROSÁRIO DE SAMPAIO. *Rui Fernandes de Almada: Diplomata português do século XVI. Resumos e Índice onomástico, top-onímico e ideográfico*. Coimbra: [Centro de Estudos Históricos da Faculdade de Letras da Universidade de Lisboa. 1973. Pp. xiv, 365-421.

GERMANY, AUSTRIA, AND SWITZERLAND

NOAKES, JEREMY, and PRIDHAM, GEOFFREY (eds. with introd.). *Documents on Nazism, 1919-1945*. New York: Viking Press. 1975. Pp. 704. \$20.00.

Die Ost- und Deutschlandpolitik der Bundesrepublikdeutschland 1969-1973: Auswahlbibliographie mit Annotationen. Bibliographien, no. 40. Bonn: Deutscher Bundestag Wissenschaftliche Dienste. 1974. Pp. v, 167.

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STERN, SELMA. *Der preussische Staat und die Juden*. Pt. 4, *Gesamtreigister zu den sieben Bänden der Teile 1-3*. Ed. by MAX KREUTZBERGER. Wissenschaftlicher Abhandlungen des Leo Baeck Instituts, 32. Tübingen: J. C. B. Mohr (Paul Siebeck). 1975. Pp. viii, 156. DM 85. See rev. of pt. 1 (1964), *AHR*, 69 (1963-64): 518.

ITALY

BERLINGUER, LUIGI. *Sui progetti di codice di commercio del regno d'Italia (1807-1808): Considerazioni su un inedito di D. A. Azuni*. Quaderni di "Studi Senesi," no. 23. Milan: Dott. A. Giuffrè Editore. 1970. Pp. 168. L. 2,300.

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MARINI, LINO. *Libertà e privilegio: Dalla Savoia al Monferrato, da Amedeo VIII a Carlo Emanuele I*. Bologna: Casa Editrice Prof. Riccardo Patron. 1972. Pp. viii, 360. L. 5,300.

MIOLO, GEROLAMO. *Historia breve e vera de gl'affari de i Valdesi delle Valli*. Ed. by ENEA BALMAS. (Storici valdesi, 1st ser., no. 3.) [Turin:] Claudiana. 1971. Pp. 153. Cloth L. 3,200, paper L. 2,500.

PARADISO, F. *Maestri ed idee nello Studio catanese dopo l'Unità*. Catania: [the author]. 1972. Pp. 277.

RUSSO, CARLA. *I monasteri femminili di clausura a Napoli nel secolo XVII*. Ricerche e documenti, no. 1. [Naples:] Università di Napoli, Istituto di Storia Medioevale e Moderna. [1970.] Pp. 154.

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EASTERN EUROPE

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CRONȚ, GHEORGHE, et al. (eds.). *Acte judiciare din Tare Românească* [Legal Sources of Romania]. Volume 9, 1775-1781. Academia de Științe Sociale și Politice a Republicii Socialiste România. Institutul de Istorie "N. Iorga." Bucharest: Editura Academiei Republicii Socialiste România. 1973. Pp. xi, 1064. Lei 60.

MARK, BER. *Uprising in the Warsaw Ghetto*. Tr. by GERSHON FREIDLIN. New York: Schocken Books. 1975. Pp. xi, 209. \$8.95.

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BARRATT, G. R. V. *Voices in Exile: The Decembrist Memoirs*. Montreal: McGill-Queen's University Press. 1974. Pp. xxi, 381. \$18.50.

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Iz istorii Sovetskoi intelligentsii (Materialy II simpoziuma po istorii rabocheho klassa, krest'ianstva i intelligentsii Sibiri) [From the History of the Soviet Intelligentsia (Materials of the 2d Symposium on the History of the Working Class, Peasantry, and Intelligentsia of Siberia)]. Akademiia Nauk SSSR, Sibirskoe Otdelenie. Novosibirsk: Nauka. 1974. Pp. 167.

KOLLONTAI, ALEXANDRA. *The Autobiography of a Sexually Emancipated Communist Woman*. Ed. with an afterword by IRING FETSCHER. Foreword by GERMAINE GREER. Tr. by SALVATOR ATTANASIO. New York: Schocken Books. 1975. Pp. xvi, 137. \$2.95.

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Recently Published Articles

At its meeting on December 27, 1974, the AHA Council authorized the Executive Director to remove the Recently Published Articles (RPA) from the American Historical Review and to implement plans for a separate publication. The Council's action was based solely on economic factors and was necessitated by the rapid and enormous increases in production costs. As of February 1976, the RPA will be published separately, though it will continue to appear, as formerly, three times a year. The RPA will be bound and have a paper cover. There will be a subscription charge, which for members of the Association will be \$5.00 for the three issues. The cost for institutions will be \$7.00 and for nonmembers \$8.00. The subscription charge will defray only some of the editorial and production costs. It is one way in which the Association will be able to continue to provide this valuable bibliography, which has no counterpart in the world. Detailed information for subscribing will be forthcoming in the near future.

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A GUIDE TO THE ABBREVIATION OF JOURNAL TITLES

GENERAL RULES: Proper names are spelled out with the exception of adjective forms and names of countries used to identify place of publication. Whenever possible, articles, conjunctions, and prepositions are deleted. Exceptions are relatively short titles such as *Past & Present* or *The Americas*.

<i>abt</i>	Abteilung	<i>archäol</i>	archäologie, archäologische,
<i>acad</i>	academia, academy		archäologischer
<i>accad</i>	accademia	<i>archeol</i>	archeologia, archeologica, archeologie,
<i>adm</i>	administration, administrative		archeology
<i>aff</i>	affaires	<i>Ariz</i>	Arizona
<i>afric</i>	africain, African, Africana, africanum	<i>Ark</i>	Arkansas
<i>afrik</i>	afrikaanse	<i>ark</i>	arkiv
<i>agrar</i>	agrarisches	<i>arq</i>	arquivos
<i>agric</i>	agricultural, agriculture	<i>arqueol</i>	arqueológico
<i>AHR</i>	<i>American Historical Review</i>	<i>art</i>	article
<i>aikakausk</i>	aikakauskirja	<i>assoc</i>	association
<i>akad</i>	Akademie	<i>assyriol</i>	assyriological, assyriologie, assyriology
<i>Ala</i>	Alabama	<i>at</i>	atti
<i>Alas</i>	Alaska	<i>Atl</i>	Atlantic
<i>alemann</i>	alemannisches	<i>AUMLA</i>	<i>Journal of the Australasian Universities</i>
<i>allg</i>	allgemein		<i>Language and Literature Association</i>
<i>allertumsk</i>	Alttertumskunde	<i>autobiog</i>	autobiography
<i>alttest</i>	alttestamentliche		
<i>Am</i>	American, Americana, Amerikas	<i>b</i>	buch (compounds only)
<i>an</i>	anales, annalen, annales, annali, annals, annua, annuaires, annual, annuarium, anuarul	<i>balt</i>	Baltic, Baltica, baltisch
		<i>bayer</i>	bayerisch, bayerischen
<i>anc</i>	ancien, ancient	<i>Beitr</i>	Beitrag, Beiträge
<i>annot</i>	annotation, annotator	<i>Ber</i>	Bericht
<i>anthol</i>	anthologica, anthology	<i>bibl</i>	bibliotek, bibliotheca, bibliothèque
<i>anthropol</i>	anthropological, anthropologie, anthropology	<i>bibliogr</i>	bibliografice, bibliographical, bibliography
		<i>bijd</i>	bijdragen
<i>antiq</i>	antiquarian, antiquarisch, antiquarischen, antiquité, antiquities, antiquity	<i>biog</i>	biography
		<i>bk</i>	book
<i>antol</i>	antología	<i>bl</i>	Blatt, Blätter
<i>antropol</i>	antropologiczny	<i>BMGN</i>	<i>Bydragen en Mededelingen betreffende de Geschiedenis der Nederlanden</i>
<i>anz</i>	anzeiger	<i>bol</i>	boletim, boletin
<i>appenzell</i>	appenzellische	<i>boll</i>	bollettino
<i>arch</i>	archiven, archives, archivio, archivo, archiv, archivum	<i>brandenburg</i>	brandenburgisch
		<i>bras</i>	Brasil, brasileira, Brazilian
<i>archaeol</i>	archaeolog, archaeologia, archaeology	<i>braunschw</i>	braunschweigisch

<i>Braz</i>	Brazilian	<i>etnol</i>	etnología
<i>brem</i>	bremisches	<i>Eur</i>	Europäische. Europas, Europe, European,
<i>Brit</i>	Britain, British		européennes
<i>bull</i>	bulletin	<i>ev</i>	evangelisch
<i>bus</i>	business	<i>explor</i>	explorations
<i>byz</i>	Byzantine, Byzantinische		
<i>cah</i>	cahiers	<i>fac</i>	faculté, faculty
<i>Calif</i>	California	<i>facx</i>	facsimile
<i>Can</i>	Canada, Canadian	<i>fak</i>	Fakulte
<i>Carib</i>	Caribbean	<i>fil</i>	filosofia, filosofiske, filozofski,
<i>cath</i>	catholic		filozofskog
<i>cent</i>	century	<i>filol</i>	filología
<i>cercet</i>	cercetări	<i>Fla</i>	Florida
<i>českoslov</i>	československý	<i>for</i>	foreign
<i>chron</i>	chronicles, chronique	<i>forsch</i>	Forschung, Forschungen
<i>circ</i>	circle, circular	<i>fr</i>	français, France, French
<i>civil</i>	civilization	<i>francisc</i>	franciscanos, franciscanum
<i>class</i>	classica, classical, classique	<i>fränk</i>	fränkische
<i>co</i>	county	<i>frankf</i>	frankfurter
<i>coll</i>	college	<i>fries</i>	friesisches
<i>collect</i>	collection, collections		
<i>Colo</i>	Colorado	<i>g</i>	giornale
<i>com</i>	comité, committee	<i>Ga</i>	Georgia
<i>comm</i>	commerce	<i>gaz</i>	gazette
<i>comp</i>	comparate, comparative, comparée	<i>gen</i>	general, général
<i>compil</i>	compilation, compiled, compiler	<i>geneal</i>	genealogist, genealogy
<i>concl</i>	conclusion	<i>geog</i>	geografi, geográfico, geographic,
<i>conf</i>	conference		geographical, géographique,
<i>cong</i>	congress		geographischen, geography
<i>Conn</i>	Connecticut	<i>Ger</i>	German, Germanic
<i>cont'd</i>	continued	<i>germ</i>	germanistisch
<i>contemp</i>	contemporaine, contemporánea,	<i>ges</i>	Gesellschaft
	contemporary	<i>gesch</i>	Geschichte, geschichtliche
<i>contrib</i>	contributed, contribution, contributor	<i>gos</i>	gospodarczych
<i>corp</i>	corporation	<i>govt</i>	government
<i>corr</i>	correspondence	<i>grad</i>	graduate
<i>c. r.</i>	comptes rendus	<i>grafsch</i>	Grafschaft
<i>crit</i>	critica, criticism, critique		
<i>cuad</i>	cuaderno	<i>h</i>	hefte (compounds only)
<i>cult</i>	cultura, cultural, culture	<i>hamburg</i>	hamburgisch
		<i>hann</i>	hannoversche
<i>D.C.</i>	District of Columbia	<i>hell</i>	hellenic, hellénique, hellenistic
<i>Del</i>	Delaware	<i>helvet</i>	helvetian
<i>demog</i>	demografie, demographische, demography	<i>hess</i>	hessisch
<i>Den</i>	Denmark	<i>Hi</i>	Hawaii
<i>dept</i>	departement	<i>hisp</i>	Hispania, hispanic, hispánicos,
<i>deux</i>	deuxième		hispanique
<i>dev</i>	developing, development	<i>hist</i>	histoire, historiae, historialinen, historical,
<i>dig</i>	digest		historická, historický, histórico, historicum,
<i>dipl</i>	diplomatic, diplomatique		historique, historisch, historisk, history,
<i>doc</i>	documentation, documents		historyczne
<i>dok</i>	dokuments		
<i>drev</i>	drevnei	<i>hohenzoll</i>	hohenzollerische
<i>dtsch</i>	deutsche, deutschen, deutsches	<i>holstein</i>	holsteinisch
<i>e</i>	east, eastern	<i>iaz</i>	iazyka
<i>ec</i>	economics, economique, economy	<i>Ida</i>	Idaho
<i>eccles</i>	ecclesiastical	<i>Ill</i>	Illinois
<i>ecles</i>	eclesiástico	<i>illus</i>	illustrated
<i>ed</i>	edited, edition, editor	<i>ind</i>	industrial, industry
<i>educ</i>	education	<i>Inda</i>	Indiana
<i>EEH</i>	<i>Explorations in Economic History</i>	<i>individ</i>	individual
<i>EHR</i>	<i>English Historical Review</i>	<i>inscr</i>	inscription
<i>ELH</i>	<i>English Literary History</i>	<i>inst</i>	institut, institute, institution, instituto,
<i>Eng</i>	English		institutului
<i>epig</i>	epigraphik, epigraphy	<i>int</i>	internacional, international,
<i>Epis</i>	Episcopal		internationale, internazionale
<i>estud</i>	estudios	<i>interdisc</i>	interdisciplinary
<i>et</i>	études	<i>intern</i>	internal
<i>ethnog</i>	ethnographisch	<i>introd</i>	introduced, introduction
<i>ethnol</i>	ethnological, ethnology	<i>ist</i>	istorii, istorijski, istoriski

<i>istruz</i>	istruzione	<i>neutest</i>	neutestamentliche
<i>ital</i>	Italian, italiana, italiano, italienisch	<i>New</i>	Nevada
<i>j</i>	journal	<i>newslett</i>	newsletter
<i>jb</i>	Jahrbuch, Jahrbücher	<i>N.H.</i>	New Hampshire
<i>JEGP</i>	<i>Journal of English and Germanic Philology</i>	<i>niedersächs</i>	niedersächsisch
<i>jugoslav</i>	jugoslovenski	<i>N.J.</i>	New Jersey
<i>jur</i>	juridical, juridiceski, juridique	<i>N.M.</i>	New Mexico
<i>kan</i>	kanonistisch	<i>no</i>	number
<i>Kans</i>	Kansas	<i>Nor</i>	Norway, Norwegian
<i>kath</i>	katholik, katholisch	<i>nord</i>	nordisk
<i>kd</i>	Kunde	<i>norm</i>	normale
<i>kl</i>	Klasse	<i>nos</i>	numbers
<i>krit</i>	kritische	<i>numis</i>	numismatic, numismatique
<i>Ky</i>	Kentucky	<i>nw</i>	northwest
<i>La</i>	Louisiana	<i>N.Y.</i>	New York
<i>landesk</i>	Landeskunde	<i>obit</i>	obituary
<i>lang</i>	language	<i>oesterr</i>	österreichisch
<i>lett</i>	letter, letterário, letteratura, letterature, lettre	<i>ok</i>	økonomie
<i>lib</i>	library	<i>Okla</i>	Oklahoma
<i>Lib Cong</i>	U.S. Library of Congress	<i>Ore</i>	Oregon
<i>libr</i>	librarian	<i>organ</i>	organization
<i>ling</i>	linguistics, linguistique	<i>orient</i>	oriental, orientale, orientalia, orientalistyczny
<i>lit</i>	literary, literatur, literature, literatury, littérature	<i>österr</i>	österreichisch
<i>lübeck</i>	lübeckische	<i>osth</i>	Osthefte
<i>lüneburg</i>	lüneburger	<i>Pa</i>	Pennsylvania
<i>mag</i>	magasin, magazine	<i>Pac</i>	Pacific
<i>marit</i>	maritime	<i>pädagog</i>	pädagogik, pädagogisch
<i>Mass</i>	Massachusetts	<i>paedagog</i>	paedagogica
<i>Md</i>	Maryland	<i>pap</i>	papers
<i>Me</i>	Maine	<i>papyrol</i>	papyrologie
<i>med</i>	medieval, médiévale, medievales, medievalia	<i>parl</i>	parlementaire, parliament
<i>meded</i>	mededelingen	<i>péd</i>	pédagog
<i>Mediterr</i>	Mediterranean	<i>pfälz</i>	pfälzische
<i>mél</i>	mélanges	<i>phil</i>	philosophical, philosophique, philosophy
<i>mém</i>	mémoires, memorial, memorie	<i>philol</i>	philology
<i>mennonit</i>	mennonitische	<i>photo</i>	photograph
<i>Mex</i>	Mexican, Mexico	<i>pol</i>	political, político, politics, Politik, politique, politische
<i>Mich</i>	Michigan	<i>pop</i>	popular
<i>mid</i>	middle	<i>port</i>	portuguesa, portuguese
<i>midcont</i>	midcontinental	<i>pres</i>	president, presidential
<i>mil</i>	militaire, militarisch, military	<i>Presb</i>	Presbyterian
<i>Minn</i>	Minnesota	<i>preuss</i>	preussisch
<i>misc</i>	miscelánea, miscellany	<i>probl</i>	problems
<i>Miss</i>	Mississippi	<i>proc</i>	proceedings
<i>mitt</i>	Mitteilung, Mitteilungen	<i>prot</i>	protestant, Protestantismus
<i>Mo</i>	Missouri	<i>prov</i>	providence, provinces
<i>mod</i>	modern, moderna, moderne	<i>PSQ</i>	<i>Political Science Quarterly</i>
<i>mond</i>	mondiale	<i>psych</i>	psychology
<i>Mont</i>	Montana	<i>pt</i>	part
<i>monum</i>	monumenta	<i>pts</i>	parts
<i>movim</i>	movimento	<i>publ</i>	publication, publishing
<i>mt</i>	mountain	<i>q</i>	quaderni, quarterly
<i>mus</i>	musée, musei, museo, museum	<i>quel</i>	quellen
<i>n</i>	north, northern	<i>r</i>	review, revista, revue, rivista
<i>nac</i>	nacional	<i>rass</i>	rassegna
<i>nass</i>	nassauische	<i>rdsch</i>	Rundschau
<i>nat</i>	national	<i>rec</i>	record
<i>nationalök</i>	nationalökonomie, nationalökonomisk	<i>rech</i>	recherches
<i>naz</i>	nazionale	<i>regist</i>	register
<i>N.C.</i>	North Carolina	<i>relig</i>	religieuse, religion, religiöse, religious
<i>N.D.</i>	North Dakota	<i>rend</i>	rendiconti
<i>ne</i>	northeast	<i>rep</i>	report, reporter
<i>Nebr</i>	Nebraska	<i>repb</i>	republic, republicii
		<i>res</i>	research
		<i>rev</i>	revolution, revolutionary
		<i>rhein</i>	rheinisch

<i>R.I.</i>	Rhode Island	<i>Tex</i>	Texas
<i>ric</i>	ricerche	<i>theol</i>	theological, theologie, theology
<i>rocz</i>	roczniki	<i>tids</i>	tidskrift, tidsskrift
<i>röm</i>	römische	<i>tyd</i>	tijdschrift
<i>roman</i>	romanische	<i>tr</i>	translated, translation, translator
<i>roy</i>	royal	<i>trans</i>	transactions
		<i>trav</i>	travail, travaux
<i>s</i>	south, southern		
<i>S.C.</i>	South Carolina	<i>u</i>	und
<i>Scand</i>	Scandinavia, Scandinavian	<i>U</i>	Universitätti, university
<i>sch</i>	school	<i>unter</i>	Unterricht
<i>schles</i>	schlesisch		
<i>schr</i>	Schrift	<i>Va</i>	Virginia
<i>schweiz</i>	schweizerisch	<i>vaterl</i>	vaterländisch
<i>sci</i>	science, scientiarum, scientific, scientist, scienze	<i>ver</i>	Verein, vereinigung, Vereins
<i>S.D.</i>	South Dakota	<i>verh</i>	Verhandlungen
<i>se</i>	southeast	<i>veröff</i>	Veröffentlichungen
<i>sec</i>	sectio, section	<i>vesn</i>	vesnik
<i>ser</i>	série, series	<i>vest</i>	vestnik
<i>slaw</i>	slawistik	<i>volksk</i>	volkskunde
<i>soc</i>	societatis, society	<i>vopr</i>	voprosy
<i>sociog</i>	sociographiques	<i>vrem</i>	vremennuk
<i>sociol</i>	sociologia, sociological, sociology	<i>Vt</i>	Vermont
<i>solothurn</i>	solothurnische		
<i>sozial</i>	sozialistischen	<i>w</i>	west, western
<i>soziol</i>	Soziologie	<i>Wash</i>	Washington
<i>Span</i>	Spanish	<i>westf</i>	westfälisch
<i>spól</i>	Spółecznuch	<i>wirtsch</i>	Wirtschaft, wirtschaftlich
<i>stat</i>	statistical, statistics, Statistik	<i>Wis</i>	Wisconsin
<i>stift</i>	stiftung	<i>wiss</i>	Wissenschaft, wissenschaftlich
<i>stor</i>	storia, storici, storico	<i>WMQ</i>	William and Mary Quarterly
<i>stud</i>	studi, studia, Studien, studies, studium	<i>württemb</i>	württembergisch
<i>sup</i>	superiore	<i>W. Va.</i>	West Virginia
<i>suppl</i>	supplement	<i>Wyo</i>	Wyoming
<i>sw</i>	southwest		
<i>Swed</i>	Sweden, Swedish	<i>yrbk</i>	yearbook
<i>symp</i>	symposium		
<i>tech</i>	technisch	<i>z</i>	Zeitschrift, Zeitschriften
<i>technol</i>	technology	<i>zeitgesch</i>	Zeitgeschichte
<i>Tenn</i>	Tennessee	<i>zgodov</i>	zgodovinski
<i>test</i>	testament, testamentum	<i>zhurn</i>	zhurnal

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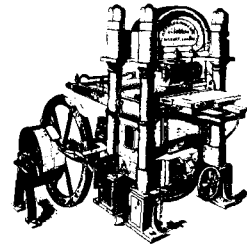
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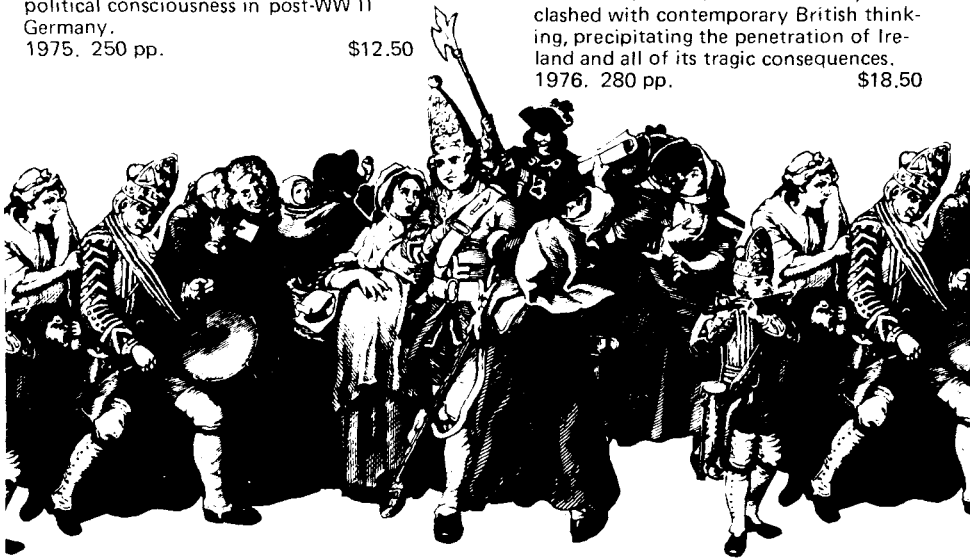
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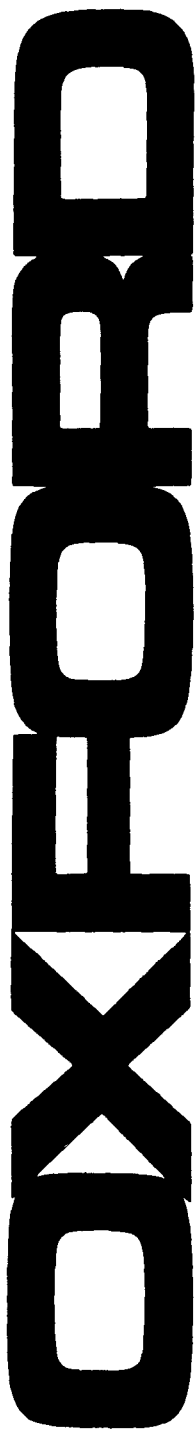
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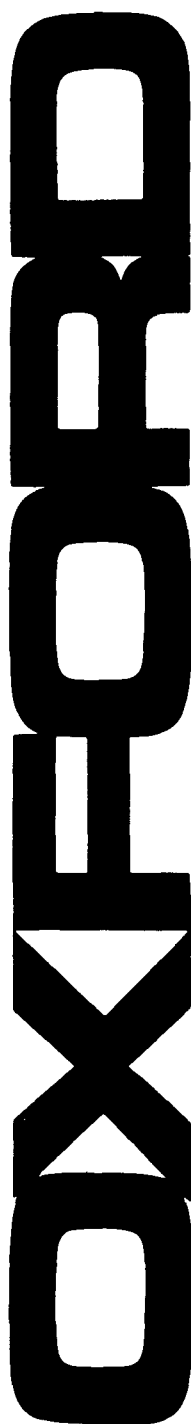
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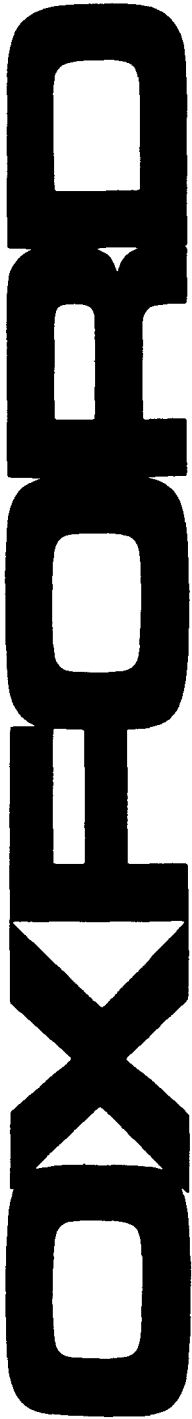
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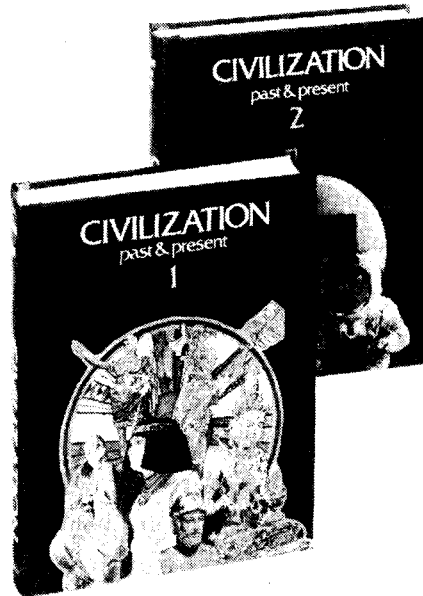
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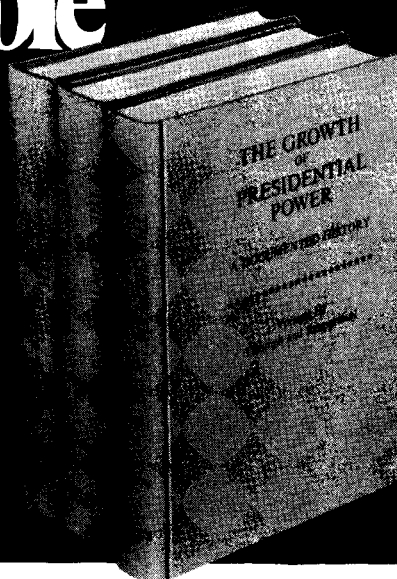
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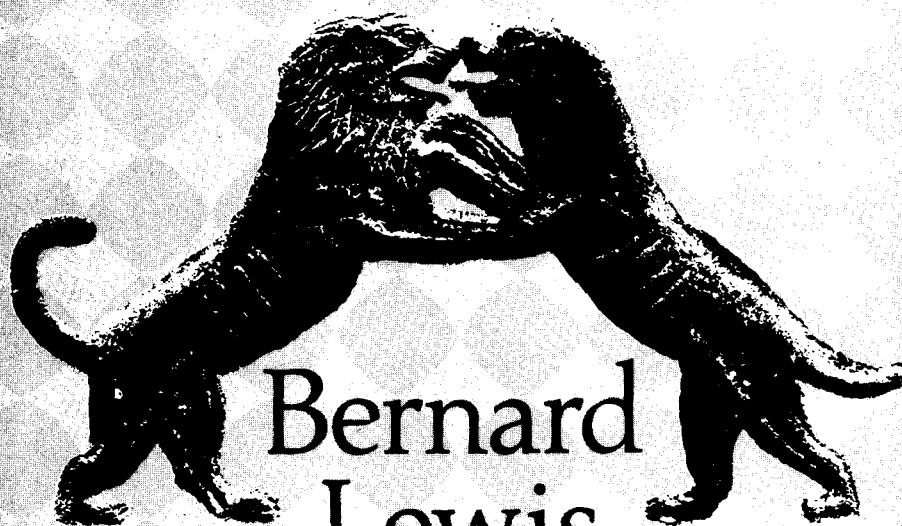
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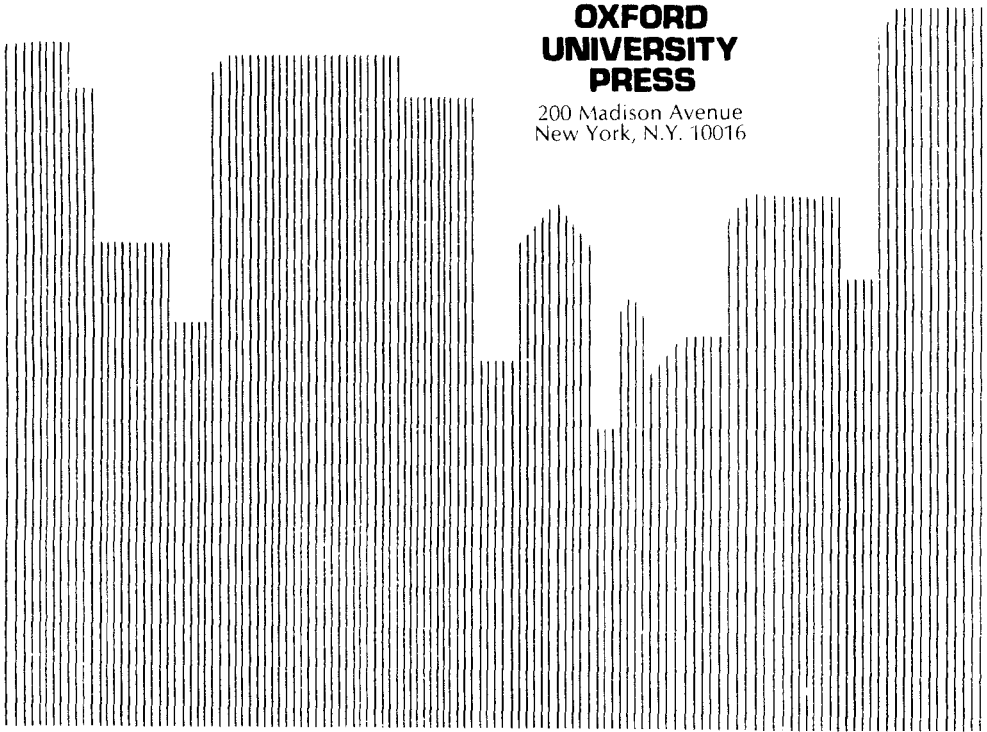
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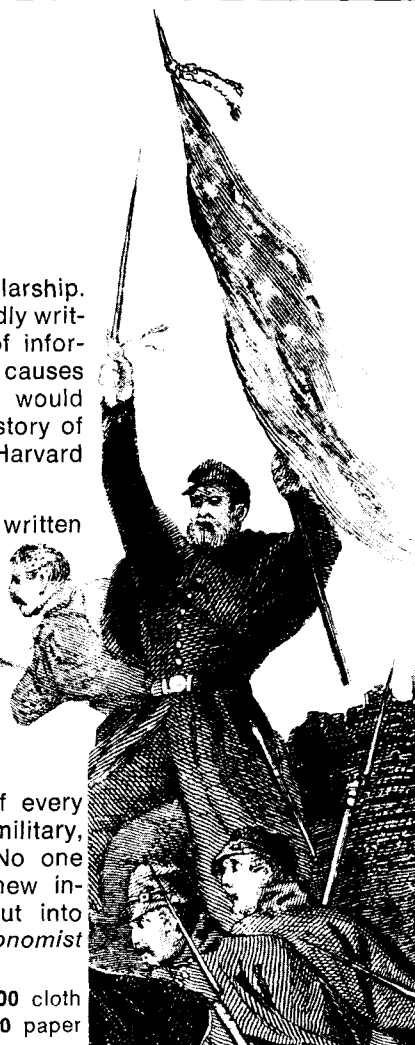
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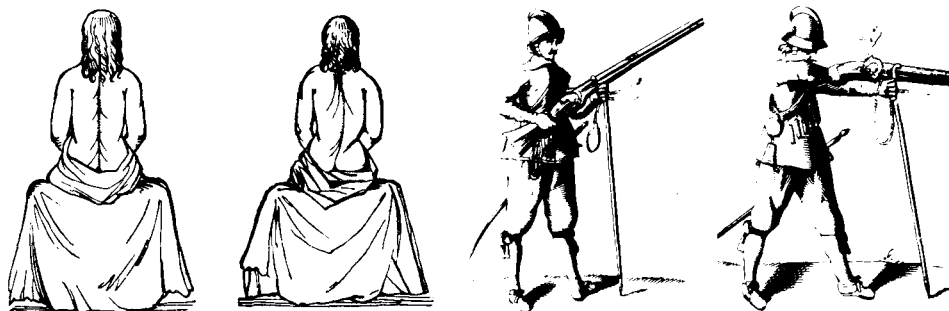
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
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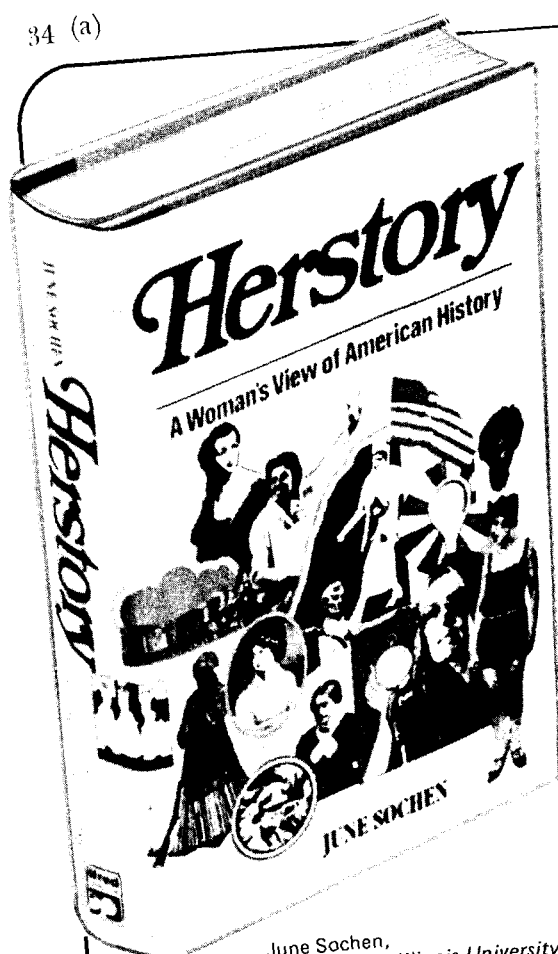
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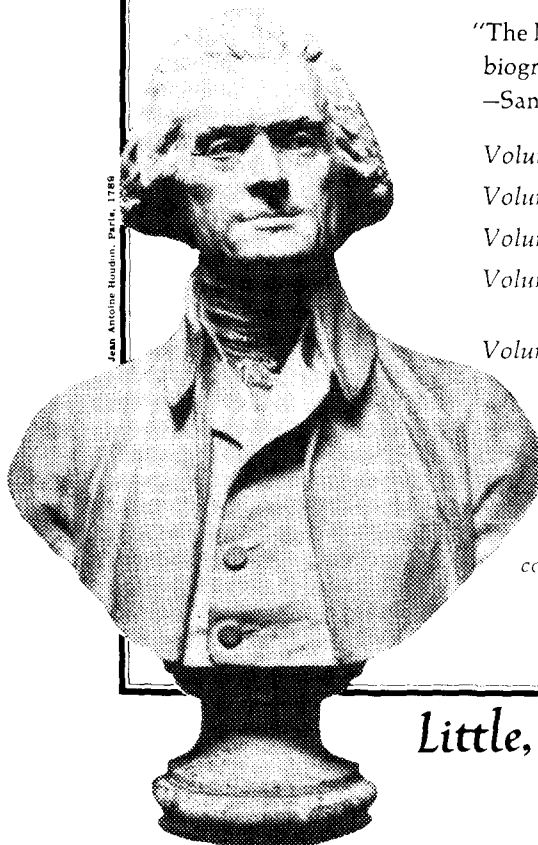
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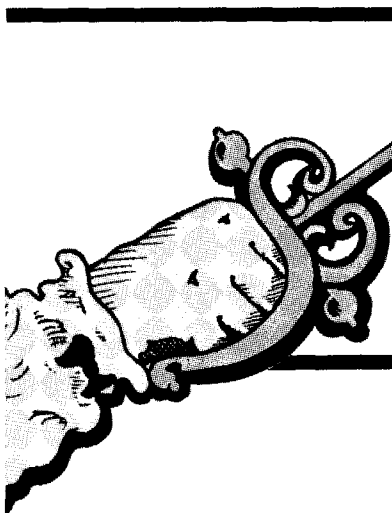
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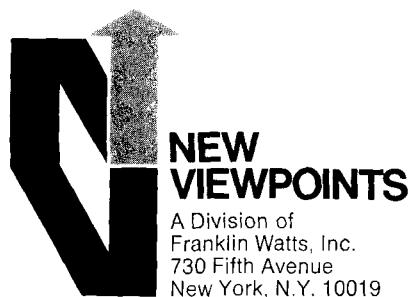
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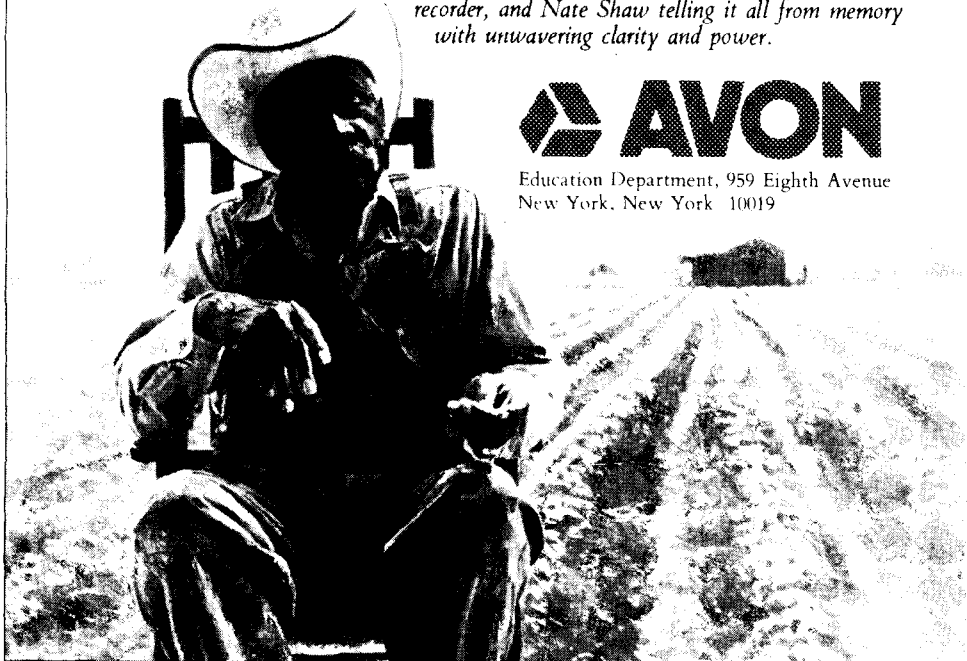
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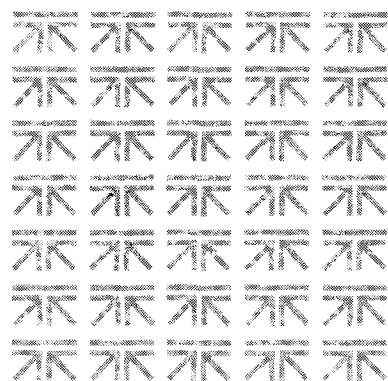
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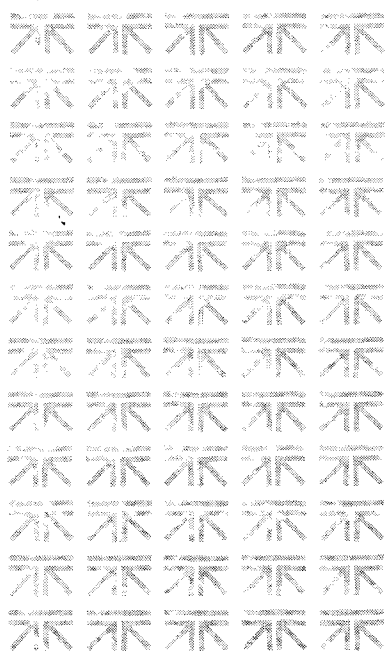


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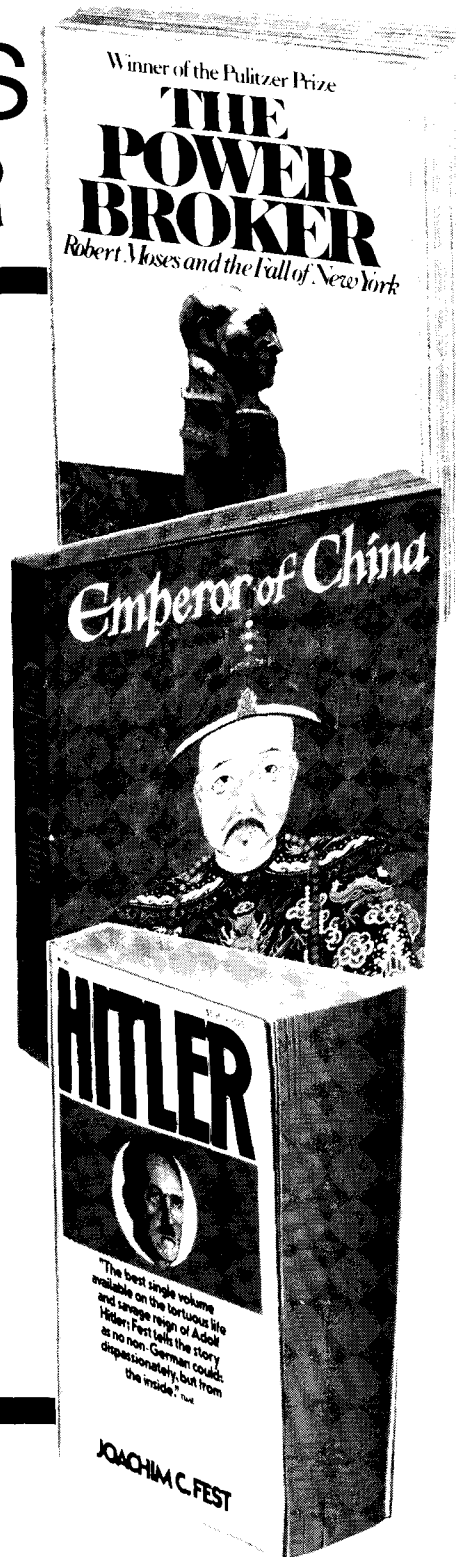
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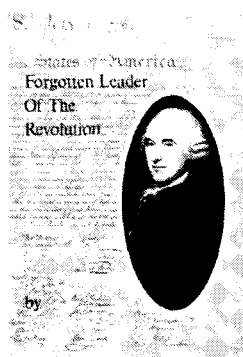
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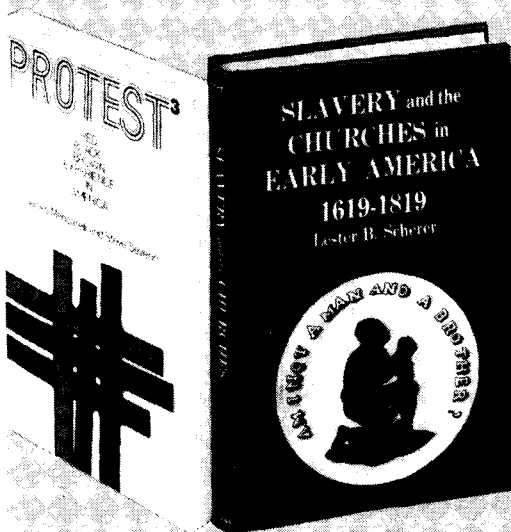
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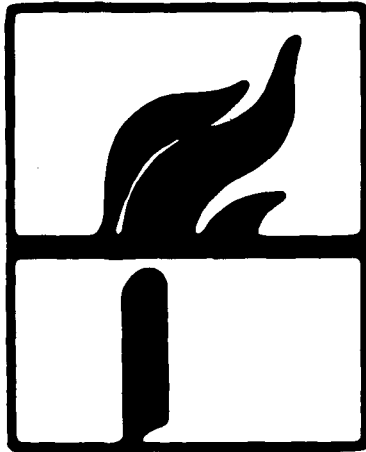
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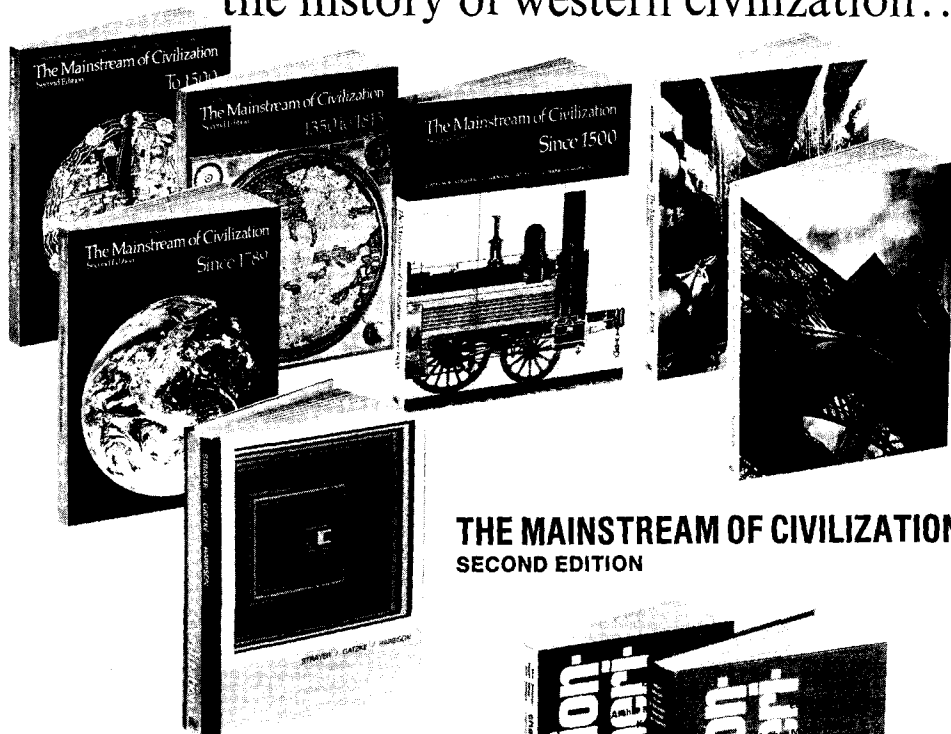
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